Marriage, minorities, and mass movements

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DOI:
10.26116/center-lis-1914

Publication date:
2019

Document Version
Publisher's PDF, also known as Version of record

Marriage, Minorities, and Mass Movements

A dissertation presented by

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Tilburg University, May 2019

Marriage, Minorities, and Mass Movements

PROEFSCHRIFT

ter verkrijging van de graad van doctor aan Tilburg University op gezag van prof. dr. G.M. Duijsters, als tijdelijk waarnemer van de functie rector magnificus en uit dien hoofde vervangend voorzitter van het college voor promoties, in het openbaar te verdedigen ten overstaan van een door het college voor promoties aangewezen commissie in de Aula van de Universiteit op maandag 1 juli 2019 om 16.00 uur door

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geboren te Fujian, China

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Acknowledgments

Almost six years ago, as a former Ph.D. student in statistics I arrived at Tilburg University with big curiosity about economics. Here I have profoundly understood "endogeneity" this word for the first time. Here I have toughly struggled with difficult economic theories and models. Here I have luckily met wonderful teachers and friends. Here I have obtained support to pay an academic visit to the London School of Economics during which I have benefited from the world-class intellectual climate and have enjoyed the globally metropolitan vibe. Here I have proudly presented my first work in economics – my Ph.D. dissertation "Marriage, Minorities, and Mass Movements". At this moment, I want to thank many people for their lesson and criticism, care and love, much and everything.

First and foremost, I am considerably grateful to my supervisors Jan C. van Ours and Arthur van Soest for their invaluable advice and guidance throughout this dissertation. Working with Jan has been not only an efficient and pleasant experience, but also a precious learning opportunity to become a qualified academic. He was always being inspiring and encouraging when I wrote new research proposals. He provided me with information on potentially appropriate data and relevant literature he had in mind. When I was stuck in programing problems of complicated models, he patiently discussed details with me and helped me to figure out possible solutions. Moreover, Jan led me into the networks of labor and applied economists by introducing various good conferences and many of his colleagues to me. I benefited substantially from participating in these conferences and communicating with these academics. Since Jan left Tilburg University, Arthur has jointly taken the responsibility for me as my another supervisor. Undoubtedly, his unique and best lectures in microeconometrics laid the foundation of my research. Arthur assisted me greatly with my job market paper from evaluation of the potential of research ideas through comparison between different model specifications to correction of my grammatical mistakes. His precise, patient, and prompt answers to econometric questions saved me a huge amount of precious time during the job market season. Both Jan and Arthur are my role models with their academic devotion as well as charming personality. I wish I will be a person like them that are not only respected as a prestigious economist but also liked as a kind person.

I want to sincerely thank my mentor outside my specific research field – Laurence van Lent. His comments and suggestions from perspectives of business and political economy facilitated to broaden the audience target and to generalize the research attractiveness of my dissertation. When I was on the job market, he taught me numerous communicating skills from which I significantly benefited in interviews, campus visits, and negotiations.

I also would like to express my gratitude to the members of my dissertation committee: Eleonora Freddi, Erik Plug, Martin Salm, and David Schindler. They devoted much of their valuable time to carefully reading my dissertation and generously offering the most detailed and constructive referee reports I had ever received. In particular, I am thankful to Eleonora Freddi and David Schindler for their elaborate instructions on the job talk with their own experience. Without their help, I would be much less confident in many skills as an academic presenter.

I have benefited considerably from insightful discussions and conversations with colleagues at Tilburg and other schools on my research and the job market. They include but are not limited to Tim Besley, Otilia Boldea, Jan Boone, Patricio Dalton, Robert Dur, Mery Ferrando, Paul Frijters, Reyer Gerlagh, Leander Heldring, Tobias Klein, Michal Kobielarz, Reto Odermatt, Gerard Padró i Miquel, Guzmán Ourens, Jan Potters, Jens Prüfer, Louis Raes, Florian Schütt, Bettina Siflinger, Dana Sisak, Florian Sniekers, Daniel Sturm, Sigrid Suetens, Burak Uras, Ben Vollaard, Bas Werker, Bert Willems, Stephane Wolton, and Noam Yuchtman. I want to extend my big thanks to Korine Bor, Cecile de Bruijn, Aislinn Callahan-Brandt, and Ella Muñoz-Baan for their strong administrative support for my overseas visits, job applications, and academic meeting reservations.

It has been my pleasure and honor to gain friendships during the period of my Ph.D. program. Haikun has been my most supportive and reliable friend. I highly enjoyed the time spent with him and our conversations on various topics. Khulan and Xiaoyu have been the greatest teaching partners and considerate officemates. As Ph.D. students in a previous cohort, Yuxin and Chen kindly provided me with many suggestions on every aspect of the Ph.D. life at Tilburg. I appreciate too the heartfelt encouragement from Mancy when I was on the job market, the regular company of Ruishen in the sports center, as well as the fun events organized by Florian and Jaime. Moreover, I will never forget how Lei, Yadi, Masha, Elisabeth, and I fought hard together for the best group of the microeconomics and macroeconomics assignments. I also enjoyed those lunches and chats with Yi, Esmée, Lucía, Christian, Karl, Tim, Sophie, Richard, Loes, Martin, Lucas, Jakub, Ali, Wanqing, Manwei, Xu, Kun, Chen, and many others.

Last but not least, I owe a debt of gratitude to my whole family especially my parents

and grandmothers. They have endeavored to offer me the best education they can since I was a child. Without their unconditional love as well as consistent support and understanding, I would not be able to pursue my dream and achieve my goal abroad for these nearly ten years. The progress in every stage of my life includes their contributions, so does this dissertation in your hand currently.

Shuai Chen

May 2019, Tilburg University

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Chapter 1

Introduction

This dissertation in applied economics studies how individuals respond in their well-being, behavior, attitudes and preferences to changes in their personal life and in society. It consists of three chapters applying economic perspectives and methodologies in the fields of labor economics and political economy. The second chapter investigates the effects of partnership dynamics on subjective well-being. The third chapter explores the symbolic functions of marriage on the stability of formal partnerships. Both chapters are with a special focus on sexual minorities. The fourth chapter, also the last chapter, studies how economic insecurity and cultural backlash have shaped the current populist attitudes and preferences, and have triggered the populist voting behavior in the United States.

The well-being effects of partnerships, especially of marriages, have been extensively studied. The positive association between partnership and well-being could originate from a causal protection effect of partnership, or merely selection, i.e. happier individuals are more likely to enter a partnership, or reverse causality such that a shock to one's current well-being induces a jump in one's future probability of partnership entry. It is of importance to disentangle these three effects. Moreover, partnerships of sexual minorities are largely unexplored. As society has become more tolerant for sexual minorities, formal partnership including registered partnership and marriage has been legalized, and public informal cohabitation has become popular among sexual minorities in many countries around the world. Thus it is interesting to investigate the influences of same-sex partnerships. There are two reasons why the sexual nature of a partnership can have different effects. First, same-sex couples may be less likely to obtain social connections and support for their partnership (Badgett, 1995; Clain and Leppel, 2001; Berg and Lien, 2002;

Carpenter, 2007; Elmslie and Tebaldi, 2007; Patacchini et al., 2015). Second, pressure from family and society may force sexual minorities to adjust their behavior (Plug et al., 2014), which in turn affects their well-being.

Chapter 2, coauthored with Jan C. van Ours, analyzes Dutch panel data to investigate whether partnership has a causal effect on subjective well-being. We take into account selection effects with an individual fixed effects model given that these selection effects are due to time-invariant unobservables such as personality. Exploring the effects of current happiness on future probability of partnership entry, we do not find evidence for reverse causality. As in previous studies, we confirm that, on average, being in a partnership improves well-being. Well-being gains of marriage are larger than those of cohabitation. We systematically compare every pair of entry and exit among different partnership transitions examining whether the effects within every pair are symmetric. We confirm symmetry between the well-being effects of partnership formation and disruption. We also find that marriage improves well-being for both younger and older cohorts, whereas cohabitation benefits only the younger cohort. Our main contribution to the literature of partnership and well-being is the special focus on same-sex partnerships. We find that these effects are homogeneous to sexual orientation. Gender differences exist in the well-being effects of same-sex partnerships: females are happier cohabiting, whereas marriage has a stronger well-being effect on males.

Furthermore, the existing economic literature of marriage has mainly focused on its practical economic incentives and benefits (Becker, 1974; Lundberg and Pollak, 2015; Pollak, 1985; Stevenson and Wolfers, 2007; Treas, 1993) and neglected its symbolic functions. In the current era of the deinstitutionalization of marriage, the practical importance of marriage has declined while its symbolic significance has still remained high and may have risen (Cherlin, 2004). The symbolic significance of marriage will not be replaced by other types of partnerships easily but keep vital in the future. Marriage enforces a unique public commitment to a long-term and even lifelong relationship, which is usually expressed in front of relatives, friends, and religious clans (Cherlin, 2004).[1] This public commitment and its resulting enforceable trust (Cherlin, 2000; Portes and Sensenbrenner, 1993) reduce the transaction costs of enforcing agreements between the partners (Pollak, 1985). Furthermore, marriage has evolved to become a marker of individual prestige and

[1] In the Netherlands, couples going to marry have to declare "in the presence of the witnesses that they accept each other as husband and wife and that they will faithfully fulfill all duties which the law connects to their marital status" (Article 67 of Book 1 of the Dutch Civil Code) and usually hold a civil ceremony and a wedding. In contrast, a registered partnership formally starts with a registration through a registrar.

personal achievement, rendering itself distinct from other types of relationships (Bulcroft et al., 2000; Cherlin, 2004).

Chapter 3, also coauthored with Jan C. van Ours, studies the effect of the symbolic significance of marriage on the stability of formal partnerships. We are interested in the stability of formal partnerships because of its benefits to the involved households and society. First, couples experience larger well-being gains from marriage than from cohabitation as well as higher happiness loss from disruption of marriage than from dissolution of cohabitation (Chen and van Ours, 2018; Kohn and Averett, 2014a,b; Stutzer and Frey, 2006). Second, children benefit more from a stable legal parent union (Pawelski et al., 2006; Prickett et al., 2015; Reczek et al., 2016). Third, stable relationships with longer duration and legally enforceable commitment (through credible punishment threats) increase fertility (Fahn et al., 2016; Gutiérrez-Domènech, 2008). Married couples anticipating a higher probability of divorce give birth to fewer children (Becker et al., 1977; Fan, 2001; Lillard and Waite, 1993). In aging societies such as most of the developed countries and China, the constantly low or further declining fertility rate has been a serious economic and demographic issue.

We exploit Dutch same-sex marriage legalization as a shock to the symbol of marital institution given that registered partnership and marriage are almost equivalent with a difference in symbolic meaning. With rich administrative data, we investigate the transition rate from registered partnership to marriage and divorce hazards from both types of relationships simultaneously. Our model allows the distinction between the effect of the symbolic significance of marriage and selection effects. We find that same-sex marriage legalization increased the divorce hazard by more than 48% for existing female partnerships and 203% for existing male ones. However, transition to marriage reduced the divorce hazard by 68% for female partnerships and 98% for male ones, which the divorce costs can explain only partly. This remarkable symbolic effect of same-sex marriage identified during the deinstitutionalization of marriage in the highly tolerant Netherlands for sexual minorities may provide common implications for marriages in general.

Societies around the world have not only witnessed the deinstitutionalization of marriage, but also been confronted with the deinstitutionalization of established politics and ideology. For a decade or more, during the Great Recession and alongside the recent immigrant influx, populism has been on the rise in many Western democracies including the U.S. (Dorn et al., 2016) and part of Europe (Colantone and Stanig, 2018b; Dustmann et al., 2017). Populism may harm the established and predictable order of politics and

the economy that has fostered economic growth and democratic norms (Rodrik, 2018b). Populism may also exert negative influences on economic performance by imprudently changing redistribution policy under political pressure (Alesina and Rodrik, 1994; Di Tella et al., 2017; Sachs, 1990), through the banking and credit system (Rousseau, 2016), and through distrust (Algan and Cahuc, 2010; Dustmann et al., 2017; Guiso et al., 2004; Knack and Keefer, 1997). There may exist situations where "economic populism" rather than "political populism" benefits the vast majority of the nation, such as significant overhaul and perhaps even erosion of established economic practices and restraints during severe economic downturns (Rodrik, 2018a). Understanding what triggers populism is important if economists and policy makers want to manage its impact.

Chapter 4 examines how economic insecurity and cultural backlash have triggered the current populism in the United States. Specifically, I exploit two quasi-natural experiments, the Great Recession and the 2014 immigration crisis, to investigate the effects of unemployment and unauthorized immigration on attitudes related to populism and populist voting in the 2016 U.S. Presidential Election. I discover that recent unemployment during the Great Recession, rather than existing unemployment from before the recession, increased the probability of attitudes forming against wealthy elites by more than 14 percentage points. Such attitudes are connected with left-wing populism. I identify perceived economic unfairness as a mechanism through which recent unemployment drove left-wing populism. However, cultural backlash rather than economic insecurity escalated by more than 12 percentage points the probability of anti-immigration attitudes developing. These attitudes are related to right-wing populism. Furthermore, I obtain evidence that cohorts economically suffering the aftermath of the Great Recession were 42 percentage points more inclined to support left-wing populist Bernie Sanders, while cohorts residing in regions most intensely impacted by the immigration crisis were 10 percentage points more likely to vote for right-wing populist Donald Trump. My study disentangles economic insecurity from cultural backlash and links each of them to a different type of populism.

Chapter 2

Subjective Well-being and Partnership Dynamics: Are Same-Sex Relationships Different? †

Abstract

We analyze Dutch panel data to investigate whether partnership has a causal effect on subjective well-being. As in previous studies, we find that, on average, being in a partnership improves well-being. Well-being gains of marriage are larger than those of cohabitation. The well-being effects of partnership formation and disruption are symmetric. We also find that marriage improves well-being for both younger and older cohorts, whereas cohabitation benefits only the younger cohort. Our main contribution to the literature is on well-being effects of same-sex partnerships. We find that these effects are homogeneous to sexual orientation. Gender differences exist in the well-being effects of same-sex partnerships: females are happier cohabiting, whereas marriage has a stronger well-being effect on males.

Keywords: Subjective well-being, Happiness, Marriage, Cohabitation, Same-sex relationships

JEL-codes: I31, J12, J16

†This chapter is based on joint work with Jan C. van Ours, which is published as Chen and van Ours (2018). We are grateful to CentERdata for making the LISS data available for this article. We also thank seminar participants at CEP Well-being, EHERO Rotterdam, Tilburg, and conference attendants at EALE 2016, ESPE 2016, as well as reviewers for their comments on a previous version of this article.

2.1 Introduction

In the past decades, a large number of studies in economics, sociology, and demography emerged on the relationship between partnership and well-being or happiness.[1] This literature predominantly asserts a positive association between marriage and well-being (Carr and Springer, 2010; Diener and Eunkook Suh, 1997; Gove and Shin, 1989; Kalmijn, 2017; Umberson and Karas Montez, 2010; Waite and Gallagher, 2000). Recently, a few studies examined whether such a positive relationship exists between cohabitation and well-being finding mixed results (Brown et al., 2005; Hansen et al., 2007; Kamp Dush, 2013; Kohn and Averett, 2014a; Musick and Bumpass, 2012; Soons and Kalmijn, 2009; Soons et al., 2009; Wright and Brown, 2017).

The positive association between partnership and well-being could originate from a causal effect of partnership on happiness. However, the positive association could also be due to selection, i.e. happier individuals are more likely to enter a partnership (Johnson and Wu, 2002; Kim and McKenry, 2002; Sandberg-Thoma and Kamp Dush, 2014; Stutzer and Frey, 2006; Waldron et al., 1996; Kalmijn, 2017; Wilson and Oswald, 2005).[2] For the causal effect there are four nonexclusive explanations. First, partnered individuals may gain from "production complementarities", i.e. specialization and division of labor (Becker, 1974, 1981; Stutzer and Frey, 2006). Second, there may be "consumption and investment complementarities" (Lundberg and Pollak, 2015; Stevenson and Wolfers, 2007). Couples may benefit from economies of scale by pooling resources, jointly consuming public goods and investing in children, and sharing leisure activities (Killewald, 2013; Waite and Gallagher, 2000). Third, a partnership may strengthen and expand social relationships. Partnered individuals do not only receive intimacy, commitment, and care from their partner, but also obtain material and emotional support from the family, relatives and friends of their partner (Dush and Amato, 2005; Ross, 1995). Last but not least, a partnership may introduce social control and mutual supervision salutary to the couple's well-being. The norms in a partnership and the daily supervision by the partner reduce possible risky behavior (Duncan et al., 2006; Fleming et al., 2010; Monden et al., 2003; Umberson, 1992).

We investigate the well-being effects of partnership dynamics in the Netherlands where there have been notable demographic changes in the past decades. In terms of partner-

[1] The literature regards subjective well-being as a substitute for happiness (Diener et al., 2009). We use the two terms interchangeably.

[2] There could be adverse selection too if individuals with inferior well-being more likely actively seek for the protection a partnership offers.

ship formation, cohabitation has become more popular at the expense of marriage. For example, by age 30, 34% of women born in the 1950s had been or were still cohabiting and 78% had been or were still married. Among women born in the 1970s, by age 30 these percentages switched to 69% for cohabitation and 45% for marriage. In the year 1998, there were about 3.4 million married couples, 0.6 million cohabiting households and 2.2 million single households. In 2016 the number of married couples decreased to 3.3 million, while the numbers of cohabiting couples and single households increased to 1.0 and 2.9 million, respectively. Furthermore, fewer cohabiting couples have made a transition into marriage. For instance, for cohabiting women aged 20-24, there is a clear drop in the probability to be married within three years after cohabitation started. For those starting to cohabit in the period 1970-1974, this probability was 58%, while for those in the period 1980-1984, it reduced to 37%, and for the 1990-1994 cohort, it further fell to 27%. In the meantime, the divorce rates have risen. In 1970 about 0.3% of all marriages dissolved, in 2014 this was about 1% (Statistics Netherlands).

Our paper exploits panel data on partnerships and subjective well-being collected in the Netherlands over the period 2008-2013. Our data allow us to make a distinction between marriage and cohabitation and between different-sex and same-sex relationships. Couples may invest different levels of tangible and intangible capital (Michael, 2004) in marriage and cohabitation (Nock, 1995; Stanley et al., 2004). Thus, the subjective well-being derived from cohabitation and marriage may be different. There are two reasons why the sexual nature of a partnership can have different effects on well-being. First, same-sex couples may be less likely to obtain social connections and support for their partnership. Although same-sex marriages have been legalized since 2001 in the Netherlands, this type of partnerships may still not be completely accepted by these couples' family, relatives, neighbors, or even employers and fellow employees (Badgett, 1995; Clain and Leppel, 2001; Berg and Lien, 2002; Carpenter, 2007; Elmslie and Tebaldi, 2007; Patacchini et al., 2015). As soon as same-sex partners start cohabiting or get married, their sexual orientation is likely to be disclosed to the public including their employers and co-workers (Plug and Berkhout, 2004). Possible discrimination and unfriendly behavior will directly harm their well-being (Mays and Cochran, 2001; Meyer, 2003; Huebner et al., 2004; Hatzenbuehler et al., 2010; McCabe et al., 2010). Second, pressure from family and society may force sexual minorities to adjust their behavior, which in turn affects their well-being. For instance, they may refuse to openly enter a partnership, be less likely to adopt a child, shy away from prejudiced occupations (Plug et al., 2014), and bear a higher risk of partnership dissolution. According to Statistics Netherlands, in

2015 over 30% of female same-sex couples married in 2005 ended up with divorce. The corresponding percentages of male same-sex and different-sex couples are 15% and 18% respectively.[3] Due to the heterogeneity of their partnership formation and stability, the effect of marital partnership on well-being may differ between same-sex and different-sex couples. The issues of the well-being and marital partnership of same-sex couples are largely unexplored in the literature.

Previous studies have investigated differences in well-being effects from marriage and cohabitation but neglected potential heterogeneity of sexual orientation. To the best of our knowledge, we are the first to investigate whether same-sex partnerships have a different effect on subjective well-being than different-sex partnerships have. Being the first country that started implementing the same-sex marriage law, the Netherlands bears the longest duration and relatively mature evolution of same-sex marriages so that its relevant data are considerably appropriate for our specific research topic. Moreover, the Netherlands is a country with a highly tolerant attitude to same-sex, bi-sexual and transgender (LGBT) individuals or sexual minorities. For example, in the Eurobarometer 2015, 91% of the Dutch respondents agreed on the statement that "same-sex marriages should be allowed throughout Europe", while the average across the 28 EU countries on this was 61% (European Commission, 2015).

We also study whether partnership effects on subjective well-being are age-cohort specific. Nowadays, older adults are more likely to be unmarried by remaining cohabiting or dating without making a formal commitment (Brown and Shinohara, 2013; Brown et al., 2006; Calasanti and Kiecolt, 2007; Cooney and Dunne, 2001; Sassler, 2010) and by increasingly divorcing (Brown and Lin, 2012; Kennedy and Ruggles, 2014). Later in life, cohabitation operates as a long-term alternative to marriage. Therefore, the positive well-being effect of cohabitation may be comparable to that of marriage for the older cohort (Brown et al., 2012; King and Scott, 2005; Vespa, 2012; Wright and Brown, 2017). However, it may also be that older adults prefer to protect the wealth they have accumulated over their lifetime rather than pool resources with their partner (Brown et al.,

[3]The differences in divorce risks between same-sex partnerships and different-sex partnerships may be attributed to different factors. Same-sex couples are less likely to have children in their household and children and the investment in children usually facilitate to stabilize a partnership (Andersson et al., 2006). Another risk factor of divorce for same-sex couples is big age difference (Noack et al., 2005). As in the Netherlands, in Norway and Sweden, same-sex marriages of women have the highest divorce risks (Andersson et al., 2006; Noack et al., 2005). The reasons for gender differences in divorce risks of same-sex partnerships are not clearly addressed in the literature. It may be because of different levels of minority stress (Meyer, 2003). Moreover, it could be that the so called "open relationship" or "open marriage" might prevail more in same-sex couples of men and less so in same-sex couples of women compared to different-sex couples.

2012). Cohabitation allows them to retain financial and economic autonomy (Brown et al., 2018; Chevan, 1996; Hatch, 1995). Moreover, older adults may be less willing to provide care-giving at later stages of their life. Cohabitation does not explicitly enforce this kind of responsibility as marriage does (Talbott, 1998). Therefore, the positive well-being effect of cohabitation could be smaller than that of marriage for older adults. Our study adds to the literature that debates whether for different age-cohorts the well-being impact of cohabitation is similar to that of marriage.

Finally, we analyze whether the well-being effects are symmetric for partnership formation and partnership dissolution. Symmetry implies that partnership formation and partnership dissolution have similar magnitudes but opposite signs. Intuitively, at the beginning of a partnership a couple is enjoying the intimacy and mutual trust (Michael, 2004) and thus partnership formation has a positive effect on well-being (Lucas et al., 2003; Lucas and Clark, 2006). However, as time goes by a partnership may be confronted with difficulties and face a breakup. Therefore, partnership dissolution may have a negative effect on the well-being of the individuals involved. Only a handful of studies examined the well-being gain of a partnership formation and the well-being loss of a partnership dissolution simultaneously. Usually, strong effects of partnership dissolution are found (Kalmijn, 2017; Simon, 2002; Strohschein et al., 2005; Williams and Umberson, 2004). However, these studies do not rigorously test whether partnership formation and dissolution have symmetric effects on well-being. Hence, our paper is one of the pioneers to systematically compare every pair of entry and exit among different partnership transitions examining whether the effects within every pair are symmetric.

All in all, our contribution to the literature on partnership and well-being is threefold. First, we establish the causal effect of marriage and cohabitation on subjective well-being. Second, our paper systematically tests the symmetry of partnership formation and dissolution. The third and also our key contribution to the literature is on well-being effects of same-sex partnerships. We confirm the results from previous studies that the well-being gains of marriage are larger than those of cohabitation. We find that these effects are homogeneous to sexual orientation. We also find gender differences in the well-being effects of same-sex partnerships. Females are happier cohabiting while marriage has a stronger well-being effect on males.

2.2 Conceptual Background

2.2.1 Theoretical Framework

Traditionally there are two competing models explaining the mechanisms through which partnership formation and partnership dissolution affect well-being: the long-term resource accumulation model and the short-term crisis adaptation model.

The long-term resource model argues that the well-being gains of partnership formation accumulate over time rather than manifest immediately. With the proceeding of a partnership, a couple keeps investing more resources in terms of shared tangible property (income, real estate, combined families and mutual friends) and intangible capital (intimacy, trust, commitment and family responsibilities) (Kamp Dush and Amato, 2005; Rhoades et al., 2011; Rusbult, 1980). This implies that the partnership ties become stronger over time and the positive well-being effect increases with partnership duration (Kalmijn, 2017; Waite and Gallagher, 2000). Likewise, the well-being losses of partnership disruption will materialize gradually. Besides, the loss of the gradually accrued investment in the previous partnership makes it difficult to recover for the divorcees' well-being (Stanley et al., 2006). Simpson (1987) shows that after breaking up a longer partnership, people feel higher level of distress over a longer period of time than individuals who break up after a short partnership. The resource model has some variants such as investment model (Rusbult, 1980), role theory (Pearlin, 1999) and chronic strain theory (Amato, 2000), all of which share the similar idea of gradual well-being promotion and deterioration in the long run during partnership formation and dissolution, respectively.

The short-term crisis adaption model asserts that the stress around a partnership disruption is only temporary and the divorcees are able to recover or adjust quickly. Thus the initial negative well-being effect will fade with the passage of time (Acock and Demo, 1994; Booth and Amato, 1991; Pearlin, 2009; Stroebe et al., 2007). Moreover, Wheaton (1990) claims that a partnership disruption, as a stressful event, actually alleviates the stress of sustaining an unsuitable partnership with low quality, so the breakup distress is only short-term. Similarly, the positive well-being effect of a partnership formation is also only temporary. Partnered individuals increase merely short-term well-being and then adapt back to the original level of well-being of pre-partnership that is determined by stable internal characteristics like personality (Anusic et al., 2014; Lucas et al., 2003; Lucas and Clark, 2006; Musick and Bumpass, 2012; Soons et al., 2009). Other variants of the crisis model include adaptation theory (Diener et al., 2006; Lucas et al., 2003), stressful-event-as-stress-relief-model (Wheaton, 1990), and setpoint theory (Anusic et al.,

2014).

The theory of the second demographic transition (Lesthaeghe, 2007) and the ideational perspective (Lesthaeghe and Surkyn, 1988) argue that in countries where citizens' physiological and safety needs have been met, society shifts to valuing self-actualization and individual autonomy. If partnerships support this kind of self-actualization and individual autonomy, partners in the union will enjoy the well-being gains; otherwise, partners will not have these well-being gains or may even have well-being declines. Similarly, Finkel et al. (2014) recently put forward that in today's modern society, young people hold increasingly high expectations and standards of marriage, such as personal growth in the marital union. The newly marrieds will feel disappointed if marriage does not catch up with their high expectations and standards of marriage, hence their well-being may not change substantially or may even decline after getting married.

2.2.2 Gender Differences

A few studies have explored gender differences in these well-being effects. Men and women seem to be affected in a similar pattern by marital statuses and transitions (Kalmijn, 2017; Strohschein et al., 2005; Williams, 2003). However, gender differences in the levels of these effects are found especially for marital dissolution in Simon (2002), Umberson (1992), and Williams and Dunne-Bryant (2006). Simon (2002) and Williams and Dunne-Bryant (2006) find that divorce entails a stronger depression for women than for men, and a more significant reduction in psychological well-being for women with young children than for their male counterparts. On the contrary, Kalmijn (2017) and Williams and Umberson (2004) allege that marital dissolution undermines life satisfaction and self-reported health for men more than for women. Blekesaune (2008) claims that divorce elevates more distress for mothers than for fathers. These differences may be attributed to different social roles of men and women in a partnership (Umberson, 1992) or simply different forms of responses to marital transitions between men and women (Simon, 2002).

Gender differences exist between marriage and cohabitation as well, although the results are quite mixed. Wright and Brown (2017) conclude that partnered men in both marriage and cohabitation enjoy similar well-being gains compared to dating and single men, while there are no considerable well-being differences among marital statuses for women. However, Brown et al. (2005) find that among middle-aged and older adults, married women have identical depression scores as cohabiting women and men do, while their scores are all higher than those of married men. Among youngsters, Mernitz and

Kamp Dush (2016) assert that direct marriage benefits emotional health for both men and women while cohabitation only benefits women, and that these gender differences are detected for first unions only. Among parents, Kamp Dush (2013) finds that after union disruption, depressive symptoms of previously married mothers, but not cohabiting mothers, return to pre-divorce levels, while depressive symptoms of previously married fathers increase more than those of cohabiting fathers. Avellar and Smock (2005) conclude that the dissolution of cohabitation entails moderate declination for men's economic situation while hurts women's economic standing much more intensely.

Cohabitation may have smaller positive effects on well-being than marriage has. The former is usually regarded as a trial marriage, so cohabitants may invest lower levels of tangible and intangible capital (Michael, 2004) in cohabitation than marrieds do in marriage (Nock, 1995; Soons et al., 2009; Stanley et al., 2004). Cohabitation, as merely a trial marriage, may exert weaker causal protective effects than marriage does in terms of production and consumption complementarities, social connections, and social controls (as discussed above). Moreover, cohabitation bears higher disruption rates and lower expectations on a stable future relationship than marriage does since it is a trial marriage. Therefore, dissolution from cohabitation may less intensely impact the well-being and emotion than that from marriage does (Blekesaune, 2008; Kamp Dush, 2013). Recovery from cohabitation disruption may be also faster than recovery from divorce.

2.2.3 Sexual Minorities

The literature on the well-being effects of different types of partnerships for sexual minorities is limited, a distinction according to gender is even more rare. According to the minority stress theory (Meyer, 2003), sexual minorities in a relationship experience stress when interacting with other people, so they respond with coping strategies including concealing their relationship (Rostosky et al., 2007). As it is harder to hide marriage than cohabitation in practice, the minority stress may shrink the well-being gap between marriage and cohabitation. Nonetheless, it is also possible that only sexual minorities with lower levels of such minority stress select themselves into marriage. Such a selection will enlarge the well-being gap between marriage and cohabitation.

Empirically, Fingerhut and Maisel (2010) claim that having a domestic partnership (closer to marriage than to cohabitation legally) alleviates negative impacts of stress on life satisfaction for sexual minorities. Riggle et al. (2010) find that sexual minorities in legally recognized relationships report less psychological distress and higher well-being

than those in committed relationships, and that a similar gap exists between those in committed relationships and singles. Wight et al. (2013) find that sexual minorities in marriage and domestic partnership have identical levels of psychological distress, which are lower than those of sexual minority singles and higher than those of different-sex married couples. Gorman et al. (2015) discover that only among different-sex couples women report significantly different physical health from men, while among sexual minorities the physical health gender differences do not exist.

2.3 Methodology Review

The methodology to establish a relationship between partnership and well-being has evolved over time as researchers have made efforts to conquer more challenging questions: going from association to causality and accounting for reverse causality. Three types of studies can be distinguished with increasing degree of complexity of the analysis. The first type of studies uses cross-sectional data focusing on correlation between partnership and well-being. Gove and Shin (1989), White (1992), Mastekaasa (1995), and Diener and Eunkook Suh (1997) conduct such an analysis for the US, Canada, Norway and multiple countries together, respectively. They confirm the positive association between subjective well-being and marriage across countries and cultures. Kurdek (1991) and Mastekaasa (1995) show that cohabitation is also positively correlated with subjective well-being in some countries. None of the studies in this category addresses the issue of causality, i.e., they do not distinguish selectivity from causality or consider possible reverse causality.

The second type of studies tries to remove the selection effect such that happier people are more likely to enter a partnership. The selection effect is due to individual-specific unobserved heterogeneity. One example of such an unobserved characteristic is personality: extroverted people may be happier and also more likely to find a partner. Ferrer-i Carbonell and Frijters (2004), Stutzer and Frey (2006), Musick and Bumpass (2012), and Averett et al. (2013) are representatives of the static fixed effects studies where individual time-invariant unobserved heterogeneity is taken into consideration. Most studies find that partnership including marriage and cohabitation increases the well-being of individuals that enter a partnership. However, this conclusion is not universal. For instance, Averett et al. (2013) show that marriage leads to a higher Body-Mass Index, to overweight and obesity as well as less exercise. Meanwhile, these studies find evidence of a positive selection effect. Chapman and Guven (2016) employ data from the US, the UK and Germany and introduce the quality of marriage as additional explanatory

variable. They discover that the positive effect of marriage on happiness is driven by happy marriages. For couples who are not happily married, marriage has a negative effect on happiness.

The third type of studies focuses on addressing potential reverse causality, i.e., a shock to the well-being of an individual leads to a jump of the likelihood of entering a partnership for that individual. Lillard and Panis (1996) employ a simultaneous-equation framework using proportional hazards for health and marital separations. The correlation of the errors of the two equations captures the selection effect. They attempt to deal with reverse causality by introducing instrumental variables in the health equation. Van den Berg and Gupta (2015) take a similar measure and claim that men generally enjoy a protection effect of marriage while women benefit from marriage only after the childbearing age. Ali and Ajilore (2011) apply propensity score matching to obtain a counterfactual outcome and correct for selection on observables. Their results show that marriage indeed reduces risky health behaviors and thus improves well-being. Kohn and Averett (2014a,b) both assume sequential reverse causality from current well-being to the partnership choice in the next period. Their first study uses a dynamic fixed effects model with internal instruments advocated by Blundell and Bond (1998) to account for reverse causality. Their second study exploits a random coefficient mixed logit model to estimate the unobserved heterogeneity associated with both health and relationship choice so that they are able to disentangle the reverse causality due to this unobserved heterogeneity. Both studies find that marriage and cohabitation benefit health similarly.

2.4 Data and Statistical Model

2.4.1 Data

Our research is based on data from the LISS (Longitudinal Internet Studies for the Social sciences) panel administered by CentERdata (see for details: www.lissdata.nl). The panel is a random sample of households drawn from the Dutch population consisting of more than 6500 households, over 10000 individuals and 93 monthly waves from November 2007 till July 2015.

With information of partnered household heads and their wedded or cohabiting partner, we identify the sexual orientation of each individual by comparing one's gender with that of his or her partner (see the Appendix 2.A for details). In this way, individuals who were always single during the period of observation are not included in part of our

analysis, i.e. where sexual orientation is included in the analysis.[4] First, we investigate the effect of any partnership on subjective well-being. Then, we study whether marriage has a different effect on subjective well-being than cohabitation has. As the society becomes more and more tolerant and people more and more open minded on the forms of partnerships, cohabitation has been considerably popular and a soaring tendency in the partnership market especially in the Netherlands (Latten and Mulder, 2014). Due to the rapid expansion of cohabitation and its distinction from other marital statuses, it is reasonable to isolate it as a different category.

There are 27,779 observations in our sample where 425 concern individuals who entered a same-sex relationship.[5] The sample size of sexual minorities is comparatively small, but it matches the estimated share of sexual minorities in the population (Sandfort et al., 2006; Bakker et al., 2009). And, in comparison with other studies our sample of sexual minorities is quite large.

Our indicator of well-being is based on the question "On the whole, how happy would you say you are?" The answer is provided on an ordinal scale from zero to ten (from totally unhappy to totally happy). Panel a of Figure 2.1 illustrates the well-being distribution by partnership status. On the happiness scale from zero to ten hardly anyone reported below five. In the relatively lower score groups of five, six and seven, non-partnered individuals dominate partnered ones in percentage, while in the higher score groups of eight, nine and ten this is the contrary. Apparently, couples are happier than non-partnered individuals. Panel b of Figure 2.1 further distinguishes marriage from cohabitation in the partnership forms. Cohabitants account for higher proportions in the happiness score groups of five, six and seven but lower proportions in the groups of eight, nine and ten than marrieds. So, generally speaking, partners are happier if they are married as compared to cohabiting. Nonetheless, the differences between various types of individuals in Figure 2.1 are all unconditional and can only be suggestive of a causal effect of partnership on evaluative happiness.

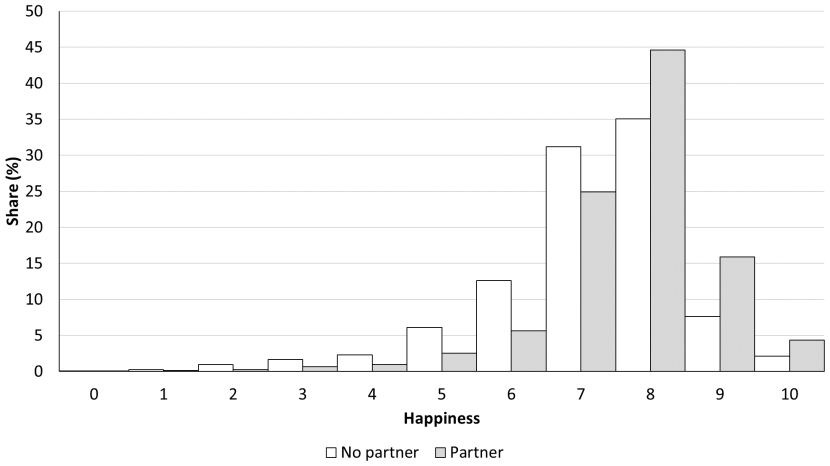
Table 2.1 gives an overview of average well-being distinguished by marital status and sexual orientation. The last column in the table confirms the findings in Figure 2.1.

[4] If the reason for remaining single is accidental, this does not bias our results. However, if the singles did not enter a partnership because they would not benefit in terms of well-being, we will overestimate the well-being effects of partnership formation. Nevertheless, it is also possible that these singles have lower well-being levels than people who experienced at least one partnership during the sample period and could have benefited more than average from partnership formation. Then, the well-being effects of partnership will be underestimated in our study.

[5] The definitions and descriptives of the relevant variables in the main models are provided in Tables 2.A.2.1 and 2.A.2.2 in Appendix 2.A.2.

Figure 2.1: Well-being and Partnership

a. Partnership



b. Marriage and Cohabitation

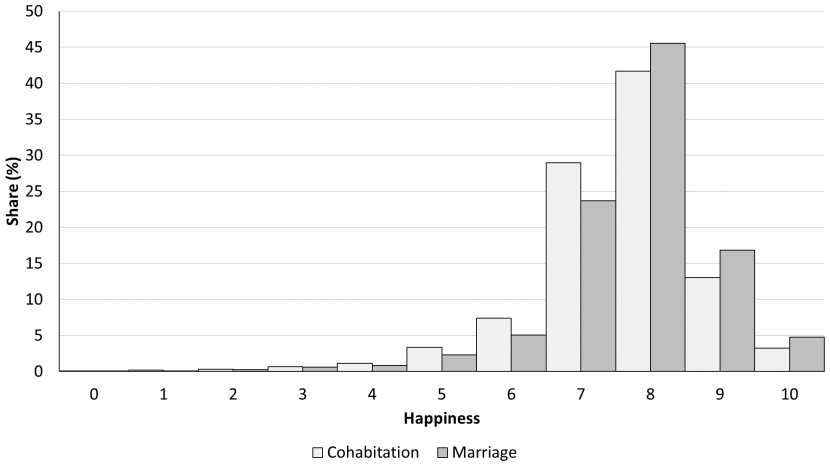


Table 2.1: Subjective Well-being by Marital Status and Sexual Orientation; Averages (Number of Observations)

	Different-sex		Same-sex		Unknown		Average	
a. Partnership								
No partner	6.98	(801)	7.65	(34)	7.14	(5,224)	7.12	(6,059)
Partner	7.73	(19,104)	7.76	(391)	7.55	(2,225)	7.71	(21,720)
b. Marriage and Cohabitation								
Marriage	7.76	(16,043)	7.83	(220)	7.81	(369)	7.76	(16,632)
Cohabitation	7.58	(3,061)	7.68	(171)	7.50	(1,856)	7.56	(5,088)

The category "unknown" exists because these individuals have always been single, or their partners did not participate in the survey if they have been ever partnered, therefore their sexual orientation cannot be identified; see Appendix 2.A for details.

On the scale from zero to ten, non-partnered individuals on average score 7.12 while partnered individuals have an average score of 7.71. On average, married couples obtain 7.76 while cohabitants have 7.56. Comparing the first two columns of Table 2.1, it is obvious that irrespective of the marital status, on average sexual minority individuals are happier although the difference is only substantial in the period when they are single. The number of observations of singles is rather small. Therefore, we make no distinction among never married, separated, divorced and widowed.[6]

The partnership transitions are displayed in Table 2.2. As shown in the table, there is a persistent stability in partnership status. Over a period of five years, among the 6,702 individuals in our sample only 614 partnership transitions happened. Transitions from cohabitation account for the largest fraction, more than twice the transitions from each of the other two marital statuses. Most of cohabitants broke up rather than entered a marriage. Over twice the number of single individuals switched to cohabitation than to marriage. Given the numbers of observations of these marital statuses in the sample, marriage is considerably more stable compared to cohabitation.

2.4.2 Statistical Model

Subjective well-being is measured on an ordinal scale from zero to ten. To account for time-invariant unobserved personal characteristics, we use a linear fixed effects model

[6]As shown in Table 2.1 our sample includes 34 observations of the single period for sexual minorities. Among them, nine observations are for the divorced phase and 25 observations are for the never married period. Of the 801 observations of the single period for sexual majority, 21 observations are for the phase of separation from a cohabitation, 330 observations are for the period of divorce from marriage, 49 for widow phase and 401 for the period of never-married. There are also 5,224 observations of singles for whom we are not able to establish sexual orientation. The latter group of singles is only used in the first part of our analysis.

Table 2.2: Partnership Transitions

	Married	Cohabiting	Single	Total
Married	–	72	61	133
Cohabiting	159	–	180	339
Single	44	98	–	142
Total	203	170	241	614

Based on 27,779 observations of 6,702 individuals over five years.

even though in such a model the dependent variable is supposed to be cardinal. As indicated by Ferrer-i Carbonell and Frijters (2004) and Stutzer and Frey (2006) when analyzing happiness and life satisfaction, the linear fixed effects model performs as well as the fixed effects ordered logit model.[7] Our model is specified as:

$$h_{it} = p'_{it}\beta_p + x'_{it}\beta_x + \alpha_i + \epsilon_{it} \tag{1}$$

where i $(i = 1, 2, ..., n)$ refer to individuals, t $(t = 1, 2, ..., T)$ stand for *years* and p is either the *partnership dummy*, or a dummy vector of different marital statuses including *married* and *cohabiting* with *single* as the reference. Furthermore, h denotes *well-being* measured on a scale from zero to ten and x represents the vector of covariates that may be correlated to both *partnership* and *well-being* such as *drinking* and *smoking behavior* (Clark and Etilé, 2006), *Body Mass Index* (Clark and Etilé, 2011) and *physical problems* (Graham et al., 2011; Kohn and Averett, 2014b), as well as demographic and socioeconomic variables like *the number of children living at home*, whether the respondent is a *home owner*, *log of personal net monthly income in Euros*, whether the respondent holds a *college diploma*, and *age-cohort dummies*. Finally, α_i represent individual-specific time-invariant effects. The error terms ϵ_{it} are assumed to have zero mean and to be independent of $p'_i = (p'_{i1}, ..., p'_{iT})$ and $x'_i = (x'_{i1}, ..., x'_{iT})$. Time-invariant unobserved heterogeneity that may affect both *partnership* and *well-being*, such as *personality*, is removed by subtracting individual sample means.

We start our analysis with a pooled cross-section analysis, ignoring individual fixed effects. Conditional on observed characteristics we estimate the association between partnership and well-being. The association combines the effect of selectivity and the causal effect from partnership to well-being. Then, by introducing individual fixed effects we remove the effect of selectivity thus establishing a causal effect. In a separate section below, we also investigate the presence of reverse causality by relating current well-being

[7] This is also the case in our analysis. By way of sensitivity analysis, we estimated a fixed effects ordered logit model finding very similar results.

to future partnership.

2.5 Parameter Estimates Subjective Well-being

2.5.1 Baseline Estimates

The relevant parameter estimates of our fixed effects model are displayed in Table 2.3. The two columns show the partnership effect on happiness for males and females separately. To indicate the importance of considering individual fixed effects, we present OLS parameter estimates in panel a.[8] There, the partnership elevates the subjective well-being by 0.60 for men and 0.45 for women, about half a point on an 11-point scale. With the fixed effects setting in panel b, partnership also has a positive effect on happiness where the difference between males and females is small. Comparing estimates of panels a and b, it is obvious that the OLS estimates are partly driven by the positive selection such that happier individuals are more likely to have a partner. Nevertheless, after removing this selection effect with the fixed effects model, there is still a significant increase in well-being related to partnership of about 0.25. So, the effect of partnership on subjective well-being and the selection effect explain around 50% of the positive association between partnership and well-being, respectively. Although well-being is measured on a scale from 0 to 10, hardly anyone reports a well-being less than 6 and few individuals report a 10. In relative terms an increase of 0.25 over a range of 6 to 9 is quite substantial.

In panel c of Table 2.3 we explore whether partnership effects are different for same-sex and different-sex couples. For males, the effect of having a same-sex partner is about the same as that of having a different-sex partner. For females, the well-being effect of having a same-sex partner is much higher than that of having a different-sex partner, but also for females, like in the case of males, we cannot reject that partnership exerts identical influences on happiness for same-sex and different-sex couples.

Panel d shows that marriage makes couples happier than cohabitation does.[9] We compare the effects of marriage and cohabitation to that of being single. Later on, we systematically analyze the dynamics or transitions among different partnership statuses. The positive effect of marriage on well-being is stronger for women than for men. The well-being effect of cohabitation is the same for both genders.

[8] We also run the OLS models on the subset of people who changed partnership status during the survey period as a robustness check since these individuals identify the fixed effects estimates. The results are similar to those in panel a of Table 2.3.

[9] We consider panel d in Table 2.3 as our baseline estimates. Appendix 2.B presents the parameter estimates of the full baseline model.

Table 2.3: Parameter Estimates Effects of Partnership on Subjective Well-being; OLS and Individual Fixed Effects

	Males		Females	
OLS				
a. Partner	0.60	(0.06)**	0.45	(0.05)**
Individual Fixed Effects				
b. Partner	0.26	(0.07)**	0.27	(0.07)**
c. Different-sex partner (β_{dsp})	0.27	(0.08)**	0.27	(0.08)**
Same-sex partner (β_{ssp})	0.25	(0.31)	0.71	(0.42)†
p-value (β_{dsp}=β_{ssp})	0.940		0.303	
d. Marriage (β_m)	0.33	(0.08)**	0.39	(0.08)**
Cohabitation (β_c)	0.21	(0.07)**	0.21	(0.07)**
p-value (β_m=β_c)	0.086†		0.004**	
e. Different-sex marriage (β_{dsm})	0.32	(0.09)**	0.44	(0.09)**
Different-sex cohabitation (β_{dsc})	0.25	(0.08)**	0.17	(0.08)*
p-value (β_{dsm}=β_{dsc})	0.351		0.000**	
Same-sex marriage (β_{ssm})	0.69	(0.41)†	0.15	(0.51)
Same-sex cohabitation (β_{ssc})	0.18	(0.32)	0.85	(0.42)†
p-value (β_{ssm}=β_{ssc})	0.094†		0.058†	

Panels a, b and d 27,779 observations of 3,088 males and 3,617 females; panels c and e 20,330 observations of 2,275 males and 2,526 females; standard errors in parentheses; † $p < 0.10$; * $p < 0.05$; ** $p < 0.01$

In panel e we distinguish different-sex and same-sex marriage and cohabitation. For different-sex partnerships the effects of marriage and cohabitation are similar to those presented in panel d. For same-sex male partnerships, the well-being effects of marriage are substantially bigger than those of cohabitation. For same-sex female partnerships, this is the opposite, i.e. the well-being effects of cohabitation are substantially larger than those of marriage.

All in all, we conclude that partnership has a positive effect on subjective well-being and that this positive effect is statistically identical for same-sex and different-sex couples. Given the significant effect of marital partnership during the short survey period of five years, our results support the idea that the well-being benefits manifest in the short term as in the crisis model (Booth and Amato, 1991; Pearlin, 2009) and adaptation theory (Diener et al., 2006; Lucas et al., 2003).

2.5.2 Reverse Causality

In the analysis of the effects of partnership dynamics on well-being, there is a possibility of selectivity or reverse causality, or both. With the linear fixed effects model, we remove selectivity due to individual-specific unobserved heterogeneity related to both partnership and happiness. However, the linear fixed effects model does not account for possible

Table 2.4: Parameter Estimates Effects of Subjective Well-being on Partnership; Individual Fixed Effects

	Partnered$_t$			
	Males		Females	
a. Happiness$_{t-1}$	-0.002	(0.005)	-0.000	(0.003)
b. Happiness$_{t-2}$	-0.003	(0.006)	0.002	(0.004)
c. Happiness$_{t-3}$	0.004	(0.007)	-0.010	(0.004)*
d. Happiness$_{t-4}$	0.004	(0.009)	-0.007	(0.006)

Standard errors in parentheses; * $p < 0.05$; covariates and constant are included in every model but not shown for parsimony.

reverse causality, i.e., the phenomenon that an individual whose happiness increases is more likely to find a partner. A person who becomes happier and more satisfied with his or her life may appear more confident and be more willing to socialize, so he or she is more attractive and approachable in the partnership market. Similarly, for a person who enters depression it is difficult to find a partner (Sandberg-Thoma and Kamp Dush, 2014).

To investigate whether or not reverse causality is an issue, we study whether single people are more likely to be partnered later on, as their happiness changes over time because of some shock. We estimate a fixed effects model in which the dependent variable is whether or not an individual is *partnered* and the independent variables are *happiness* in an earlier period and the same covariates as before. If reverse causality existed, we would expect that a higher level of happiness makes partnership formation later on more likely. We use different lags for happiness to allow for effects that materialize quickly or more slowly. Table 2.4 displays the relevant parameter estimates of lagged happiness. Row a shows that a positive shock to happiness of an individual who was single does not improve his or her probability to enter a partnership one year later. Rows b to d present that also after two, three or four years there is no effect. None of the results are sizable or significant except the coefficient in row c for women. Although it is significant at 5% significance level, the magnitude of 1% is still negligible. From this we conclude that reverse causality from happiness to future partnership dynamics is not an issue.

2.5.3 Symmetry

Partnership formation and partnership disruption may have different effects on subjective well-being both in sign and magnitude. Therefore, it is interesting to distinguish between entering a partnership and quitting it and test whether their effects are symmetric. We introduce a "single to partnered" dummy variable with value one in case of partnership

Table 2.5: Parameter Estimates Effects of Partnership on Subjective Well-being; Asymmetry of Partnership Formation and Dissolution

	Males		Females	
a. Single to partnered (β_{sp})	0.18	(0.09)†	0.17	(0.10)
Partnered to single (β_{ps})	-0.30	(0.09)**	-0.29	(0.08)**
p-value (β_{ps}=$-\beta_{sp}$)	0.339		0.351	
b. Single to married (β_{sm})	0.17	(0.16)	0.28	(0.20)
Married to single (β_{ms})	0.25	(0.15)	-0.00	(0.13)
p-value (β_{sm}=β_{ms})	0.722		0.249	
Single to cohabiting (β_{sc})	0.06	(0.11)	0.05	(0.12)
Cohabiting to single (β_{cs})	-0.18	(0.10)†	-0.14	(0.09)
p-value (β_{cs}=$-\beta_{cs}$)	0.418		0.561	
Cohabiting to married (β_{cm})	0.06	(0.10)	0.08	(0.09)
Married to cohabiting (β_{mc})	-0.31	(0.15)*	-0.02	(0.11)
p-value (β_{cm}=$-\beta_{mc}$)	0.152		0.660	
p-value ($\beta_{sm}-\beta_{ms}$=$\beta_{sc}+\beta_{cs}$=$\beta_{cm}+\beta_{mc}$=0)	0.429		0.599	

Column 1 contains 12,955 observations of 3,088 men; column 2 14,824 observations of 3,617 women. Standard errors in parentheses; † p < 0.10; * p < 0.05; ** p < 0.01.

formation and value zero otherwise. Likewise, the "partnered to single" dummy values one in case of partnership dissolution and values zero otherwise.

Panel a of Table 2.5 presents seemingly asymmetric effects during partnership formation and during partnership dissolution. The first term of single to partnered refers to the effect when a partnership forms and the second stands for the effect when a partnership dissolves. In both columns, partnership formation and disruption have opposite effects on the subjective well-being for both men and women. For example, males who make a transition from singleness to partnership experience on average an increase in well-being of 0.18. If they break up and become single, they face a decrease in well-being of 0.30. In order to formally check whether the effects are identical in magnitude during partnership formation and disruption, we conduct the pair symmetry test with the null hypothesis such that the absolute values of the coefficients of the two transition variables are equal. The p-value of the test indicates that we cannot reject that the effects are symmetric.

Partnership is heterogeneous in the sense that it includes informal cohabitation and formal marriage. The subjective well-being derived from cohabitation and marriage is likely to be different. That is why we further investigate the symmetries of transitions among marriage, cohabitation and singleness. Panel b displays the effects on subjective well-being of several types of partnership dynamics. For example, entering marriage does not seem to raise subjective well-being for cohabiting couples while going from marriage to cohabitation significantly reduces men's happiness while it does not affect women's hap-

piness.[10] Marriage provides a tighter, more socially recognized and enforceable contract than cohabitation. Apparently, for males this is more of an issue than for females. Nevertheless, for these more elaborate dynamics among singleness, cohabitation and marriage, though the symmetries still hold, most of the estimates are insignificant. This may be due to the small number of observations in each transition (see Table 2.2). The estimation of the partnership dynamics also provides evidence to the short-term crisis model or adjustment theory. During partnership formation, subjective well-being improves quickly; during partnership dissolution, subjective well-being is harmed immediately as well.

2.5.4 Age Cohort Differences

For younger and older individuals, marital partnership may have different meanings. For instance, among youngsters, cohabitation is usually seen as a trial marriage, while older individuals may think of cohabitation as a long-term substitute for marriage (Brown et al., 2012; King and Scott, 2005; Vespa, 2012; Wright and Brown, 2017).

To investigate potential heterogeneity in the effects of partnership on well-being, we explore whether there are differences by age. Kohn and Averett (2014b) distinguish individuals under 45 and over 45 and indeed find different relationship effects for the two sub-samples. Following their idea, we divide the sample into two age cohorts: people born before 1962 (46-year old in the first wave 2008 of the survey) and after 1962. The relevant parameter estimates are displayed in Table 2.6. Panel a shows that partnership increases happiness for men born before 1962 but not for women in the same age cohort. Both men and women in the older cohort obtain larger well-being gains from marriage than from cohabitation. Panel b displays that partnership exerts a positive influence in the younger cohort and so do marriage and cohabitation. For the younger cohort, the happiness benefits from marriage are bigger than those from cohabitation but the difference is not statistically significant.

These findings raise an interesting question: why does cohabitation benefit only the younger age cohort but not the older one? We speculate that older adults may prefer to protect the wealth they have accumulated over their lifetime rather than pool the resources with their partner (Brown et al., 2012), and cohabitation allows them to retain financial and economic autonomy that would not be possible in marriage (Brown et al., 2018; Chevan, 1996; Hatch, 1995). Furthermore, older adults, especially older women, may be less wiling to provide care-giving at a later stage of their life, and cohabitation

[10]Interpreting these parameter estimates should be cautious since due to data limitations we ignore partnership transitions within a year.

Table 2.6: Parameter Estimates Effects of Partnership on Subjective Well-being by Age Cohort

	Males		Females	
a. Born before 1962				
1. Partner	0.28	(0.12)*	0.17	(0.15)
2. Marriage (β_m)	0.36	(0.12)**	0.31	(0.16)*
Cohabitation (β_c)	0.13	(0.14)	-0.10	(0.17)
p-value (β_m=β_c)	0.044*		0.000**	
b. Born in 1962 or thereafter				
3. Partner	0.25	(0.09)**	0.30	(0.08)**
4. Marriage	0.30	(0.11)**	0.37	(0.10)**
Cohabitation	0.23	(0.09)**	0.28	(0.08)**
p-value (β_m=β_c)	0.515		0.313	

Panel a 15,395 observations with 1,704 men and 1,773 women;
panel b 12,384 observations with 1,385 men and 1,845 women.
Standard errors in parentheses; † $p < 0.1$; * $p < 0.05$; ** $p < 0.01$

does not explicitly enforce this kind of responsibility as marriage does (Talbott, 1998). Another possible explanation is that for people born before 1962, cohabitation was still not widely accepted when they entered the partnership market. The social attitude to cohabitation may have also influenced their individual attitude. Even though later they chose to cohabit, they still did not regard cohabitation as similar to marriage. On the contrary, when individuals in the younger age cohort entered a partnership, society already bore quite a tolerant attitude to cohabitation. In the mean time cohabitation is more popular than marriage in the partnership market.

2.6 Conclusions

Many studies find positive well-being effects of a partnership for which there are various explanations. It may be that partnered individuals gain from production complementarities, division of labor or consumption and investment complementarities. It may also be that couples benefit from economies of scale by pooling resources, jointly consuming public goods and investing in children, and sharing leisure activities. A partnership may strengthen and expand social relationships. Finally, a partnership may introduce social control and mutual supervision.

We analyze Dutch panel data to investigate whether there is a causal effect of partnership on subjective well-being finding that this is indeed the case. We do not find evidence for reverse causality which occurs if a positive shock to one's well-being induces partnership formation. As in a few previous studies, we find that well-being gains of

marriage are larger than those of cohabitation which may be related to different investment levels of tangible and intangible capital. We also find that the well-being effects of partnership formation and disruption are symmetric. Since our panel covers a five-year period, this finding supports the crisis model and adaptation theory that the well-being effects of marital partnership transitions manifest in the short term rather than that they need a long time to accumulate. Furthermore, we find that marriage improves well-being for both younger and older cohorts while cohabitation only benefits younger cohort. This may be due to the weaker desire of pooling economic resources and lower willingness of care-giving for older cohabitants. Or, it might be because of different social acceptance of cohabitation when older individuals initially entered the partnership market a long time ago. Even though they later on chose to cohabit, older individuals still do not regard cohabitation as similar to marriage.

Whereas we contribute to the literature by studying partnership dynamics, investigating reverse causality and establishing cohort-specific differences in well-being effects, our main contribution is on well-being effects of same-sex partnerships. We find that these effects are similar to those of different-sex partnerships. This may seem surprising because of possible discrimination against sexual minorities once their sexual orientation is disclosed. Perhaps thanks to the effective implementation of education and policy on marriage equality and respect for sexual minorities, this prejudice against sexual minorities does not prevail in the Netherlands. Although overall same-sex and different-sex partnerships have similar effects on well-being we do find gender differences in the well-being effects of same-sex partnerships. Females are happier cohabiting while marriage has a stronger well-being effect on males. We can only speculate about the reasons for this difference as the literature on the well-being effects of different types of partnerships for sexual minorities is limited. It might be that especially for male same-sex partnerships marriage provides a tighter, more socially recognized and enforceable contract than cohabitation. Apparently, for female same-sex partnerships this is less of an issue.

We are confronted with a few difficulties in the current study. First, the analysis is restricted to a short panel, so we are unable to examine whether the well-being effects of partnership dynamics will persist in the long term. Basically, the crisis model argues that these effects are temporary while the resource model claims that these effects need a long time to materialize. To investigate which of the models is more realistic, a longer panel is more helpful. Second, to analyze the heterogeneity of sexual orientation in the well-being effects of partnership dynamics in more detail, a larger dataset is needed. The number of partnership transitions and the size of same-sex sample are still relatively small

in our data. Due to these limitations, our parameter estimates for same-sex partnerships are imprecise. Third, though we include a number of time-varying covariates and apply the fixed effects model to account for time-invariant unobservables, we cannot completely resolve the concern of the possible time-varying confounding unobservables. If the panel data contained information on the nature and magnitude of exogenous shocks to partnership market, we would be able to exploit such a shock to draw a more compelling causal conclusion.

Appendix 2.A: Details on Our Data

2.A.1: Sexual Orientation

It is hard to measure an individual's sexual orientation in surveys. There are basically three ways to do this and each method has its limitations. The first method is simply asking for sexual preferences: "Regarding your sexual preference, are you attracted to men or to women?" Answers could be in five categories: one only to men; two especially to men, but to some extent also to women; three as much to men as to women; four especially to women, but to some extent also to men; five only to women. This measure was employed by Plug and Berkhout (2004), and Buser, Geijtenbeek, and Plug (2015). The second measure of sexual orientation is through sexual activity. Badgett (1995) and Black et al. (2003) used answers to the question "How many males and females did you have sex with?" The third measure of sexual orientation is based on the gender of respondents' partner. This measure was used by Klawitter and Flatt (1998) and Allegretto and Arthur (2001).

The three measures of sexual orientation have their own advantages and shortcomings: sexual preference and past sexual activity ask directly about sexual orientation so they can identify sexual orientation with just cross sectional data even for respondents who are single at the time of the survey. However, they may result in plenty of non-responses because of privacy. Besides, past sexual activity will probably wrongly classify, for example, individuals who participated in different-sex activities a few times but then figured out they prefer same-sex relationships. Data of the gender of respondents' partner are more widely accessible than sexual preference and past sexual activity. Moreover, sexual orientation based on partner's gender is more observable to the respondents' family and employers. Thus, if the researchers want to investigate outside influence related to sexual orientation, this measure is more appropriate. Nevertheless, for respondents who were partnered in none of the waves of the panel, this measure can not detect their sexual orientation. This may lead to sample selection (Plug and Berkhout, 2004). The three measures capture different respects of sexual orientation hence are not necessary to be completely consistent. Which measure to use empirically depends on the specific problem to be investigated. We study the effect of partnership on subjective well-being where in part of our analysis we distinguish between different-sex and same-sex relationships. Since such an effect is directly related to the respondents' partner during the partnership, the measure of sexual orientation based on partner's gender is most suitable.

From the background variables in the LISS-panel, we know the position within the

household of each of the respondents, i.e., whether they are household head, wedded partner, cohabiting partner, parent (in law), child living at home, house mate, and family member or boarder. We also know marital status which includes never married, married, separated, divorced, and widowed. Information on the domestic situation includes single without child(ren), single with child(ren), (un)married cohabitation without child(ren), (un)married cohabitation with child(ren), and other. With these variables we are able to identify the sexual orientation of every household head and their partner.

First, we combine the originally 93 monthly waves to construct an initial panel. Second, in the initial panel we keep only the partnered household heads and their (un)wedded partner using the categories of (un)married cohabitation with(out) child(ren) in "domestic situation". Third, we identify the sexual orientation of every partnered individual by comparing one's gender with that of one's (un)wedded partner and record the corresponding person numbers in the same-sex group and different-sex group respectively.[11]

2.A.2: Definitions and Descriptives of Variables

The subjective well-being indicator is collected annually, while other variables including the partnership dynamics are available on a monthly basis. In our analysis all variables are specified on an annual basis. This means some loss of information, for example, multiple changes in partnership status within a year are ignored. Table 2.A.2.1 provides an overview of the definition of the variables we use in our analysis. Table 2.A.2.2 presents the descriptives of these variables.

[11]There are two exceptions, bisexuals and trans-genders, which consist of 30 individuals together. Following previous studies (Plug et al., 2014; Buser et al., 2015) we categorize them into same-sex group since they all belong to sexual minorities. In the interpretation and discussion we will use the expressions of same-sex and sexual minorities interchangeably.

Table 2.A.2.1: Definitions of Variables

Variable	Definition
Subjective well-being	"On the whole how happy would you say you are?" (score 0-10)
Partnered	Dummy variable if partnered
Married	Dummy variable if married
Cohabiting	Dummy variable if cohabiting
Single	Dummy variable if never married, separated, divorced or widowed
Same-sex	Dummy variable if classified into same-sex group
Children number	Number of living-at-home children
Home owner	Dummy variable if home owner
Net income	Personal net monthly income in Euros
Missing info net income	Dummy variable if net income is missing
College	Dummy variable if with college diploma
Drinking	Dummy variable if drink alcohol during the last seven days
Drinking days	Number of days in the past seven days drink alcohol
Smoking	Dummy variable if smoke now
BMI	Body Mass Index
Physical problem	Number of physical problems diagnosed by physicians
Missing info physical problem	Dummy variable if physical problem is missing
Age20–70p	Age cohort dummies, reference cohort is teenagers

Table 2.A.2.2: Descriptives

	Men			Women		
Variable	Mean	Minimum	Maximum	Mean	Minimum	Maximum
Subjective well-being	7.6	0	10	7.6	0	10
Number of children	0.8	0	7	0.9	0	7
Net income/10^4	0.2	0	16.3	0.1	0	28.6
Drinking days	2.8	0	7	1.9	0	7
BMI	25.7	13.9	64.4	25.4	12.4	81.4
Physical problem	0.8	0	10	0.8	0	18
Percentages						
Partnered	80.7	0	100	76.0	0	100
Married	62.8	0	100	57.4	0	100
Cohabiting	18.0	0	100	18.6	0	100
Single	19.3	0	100	24.0	0	100
Home owner	75.7	0	100	72.8	0	100
Missing info net income	5.0	0	100	5.3	0	100
College	34.0	0	100	26.8	0	100
Drinking	73.4	0	100	56.1	0	100
Smoking	21.3	0	100	18.4	0	100
Missing info physical problem	5.2	0	100	5.0	0	100
Different-sex	74.1	0	100	69.5	0	100
Same-sex	1.4	0	100	1.7	0	100
Unknown orientation	24.5	0	100	28.8	0	100
Age to 19	4.3	0	100	5.2	0	100
Age 20 to 29	8.3	0	100	10.6	0	100
Age 30 to 39	12.8	0	100	15.2	0	100
Age 40 to 49	17.9	0	100	18.9	0	100
Age 50 to 59	20.5	0	100	21.1	0	100
Age 60 to 69	23.0	0	100	18.8	0	100
Age 70 plus	13.2	0	100	10.2	0	100

Based on 12,955 observations of 3,088 men and 14,824 observations of 3,617 women.

Appendix 2.B: Parameter Estimates Baseline Model

Table 2.B.1 presents a full set of parameter estimates related to Table 2.3 panel d. The first two rows indicate the effects of marriage and cohabitation, identical to the ones presented in Table 2.3 panel d. Teenagers (the reference of the age group dummies) appear to have the highest level of happiness. The happiness of men aged 20 to 29 is somewhat lower while from age 30 onward well-being drops even further. However, for females the age gradient is hardly present. The number of children has a negative effect on happiness although only for females this effect is significantly different from zero. Net income has a positive effect on happiness for males but not for females. Physical problems have a negative happiness effect for males and smoking has a positive effect for males. Most of the other variables have no significant effect on happiness.

Table 2.B.1: Parameter Estimates Effects of Partnership on Subjective Well-being; Full Baseline Model

	Males		Females	
Marriage	0.33	(0.08)**	0.39	(0.08)**
Cohabitation	0.21	(0.07)**	0.21	(0.07)**
Children number	-0.04	(0.03)	-0.07	(0.03)**
Home owner	-0.08	(0.07)	-0.02	(0.06)
Log(net income)	0.04	(0.01)**	-0.00	(0.01)
Missing info net income	0.30	(0.12)*	-0.19	(0.09)*
College	0.09	(0.08)	0.10	(0.08)
BMI	0.01	(0.01)	-0.00	(0.00)
Physical problem	-0.03	(0.01)*	-0.02	(0.01)
Missing info physical problem	-0.02	(0.04)	-0.00	(0.04)
Smoking	0.09	(0.04)†	0.04	(0.05)
Drinking	-0.02	(0.03)	0.03	(0.03)
Drinking days	-0.00	(0.01)	-0.01	(0.01)
Age 20 to 29	-0.13	(0.08)†	-0.11	(0.07)†
Age 30 to 39	-0.34	(0.12)**	-0.14	(0.10)
Age 40 to 49	-0.45	(0.13)**	-0.15	(0.11)
Age 50 to 59	-0.56	(0.14)**	-0.13	(0.12)
Age 60 to 69	-0.44	(0.15)**	-0.09	(0.13)
Age 70 plus	-0.39	(0.16)*	0.05	(0.15)
Constant	7.37	(0.21)**	7.62	(0.16)**

12,955 observations of 3,088 men; 14,824 observations of 3,617 women.
Standard errors in parentheses; † $p < 0.1$; * $p < 0.05$; ** $p < 0.01$

Chapter 3

Symbol Matters Little but for Marriage: Same-Sex Marriage Legalization and Partnership Stability

Abstract

The practical economic importance of marriage has declined while its symbolic significance has still remained high and may have risen. We study the effect of the symbolic significance of marriage on the stability of formal partnerships. We exploit Dutch same-sex marriage legalization as a shock to the symbol of marital institution given that registered partnership and marriage are almost equivalent with a difference in symbolic meaning. With rich administrative data, we investigate the transition rate from registered partnership to marriage and divorce hazards from both types of relationships simultaneously. Our model allows the distinction between the effects of interest and selection effects. We find that same-sex marriage legalization increased the divorce hazard by more than 48% for existing female partnerships and 203% for existing male ones. However, transition to marriage reduced the divorce hazard by 68% for female partnerships and 98% for male ones, in which the divorce costs may explain only part. This remarkable symbolic effect of same-sex marriage identified during the deinstitutionalization of marriage in the highly tolerant Netherlands for sexual minorities may provide common implications for marriages in general.

Keywords: Same-sex marriage, Registered partnership, Divorce, Duration analysis
JEL-codes: D78, J12, J15, J16, K36

This chapter is based on joint work with Jan C. van Ours. We thank Statistics Netherlands (CBS) for making their restricted access micro-data available for analysis through a remote access facility. We are also grateful to Eleonora Freddi, Erik Plug, Martin Salm, David Schindler, Arthur van Soest, seminar participants at Tilburg and conference attendants at EEA/ESEM 2017, ENTER Jamboree 2017, ESPE 2017, and Netherlands Economists Day 2017 for their helpful comments and suggestions on a previous version of this paper.

3.1 Introduction

In the current era of the deinstitutionalization of marriage, the practical importance of marriage has declined while its symbolic significance has still remained high and may have risen (Cherlin, 2004). Formal relationships such as marriage have become less popular while informal relationships such as cohabitation have been on the rise. Some countries have introduced a registered partnership which is legally similar to marriage as another type of formal relationship. Moreover, the previous marital privilege of different-sex couples has been extended to be available for same-sex partners in many countries around the world. Most of the practical functions of marriage, such as specialization or division of labor and economies of scale by pooling resources (Becker, 1974; Pollak, 1985; Treas, 1993) as well as consumption and investment complementarity (Lundberg and Pollak, 2015; Stevenson and Wolfers, 2007), and their induced gains can be achieved through registered partnership or cohabitation. However, the symbolic significance of marriage will not be replaced easily but remain vital in the future. Marriage enforces a unique public commitment to a long-term and even lifelong relationship, which is usually expressed in front of relatives, friends, and religious clans (Cherlin, 2004).[1] This public commitment and its resulting enforceable trust (Cherlin, 2000; Portes and Sensenbrenner, 1993) reduce the transaction costs of enforcing agreements between the partners (Pollak, 1985). Furthermore, marriage has evolved to become a marker of individual prestige and personal achievement, rendering itself distinct from other types of relationships (Bulcroft et al., 2000; Cherlin, 2004).

We study how the symbolic significance of marriage, i.e. the unique public commitment and the symbol of individual prestige and achievement of marriage, affects the stability of formal partnerships. We are interested in the stability of formal partnerships because of its benefits to the involved households and society. First, couples experience larger well-being gains from marriage than from cohabitation as well as higher happiness loss from disruption of marriage than from dissolution of cohabitation (Chen and van Ours, 2018; Kohn and Averett, 2014a,b; Stutzer and Frey, 2006). Second, children benefit more from a stable legal parent union (Pawelski et al., 2006; Prickett et al., 2015; Reczek et al., 2016). Third, stable relationships with longer duration and legally enforceable commitment (through credible punishment threats) increase fertility (Fahn et al., 2016;

[1] In the Netherlands, couples going to marry have to declare "in the presence of the witnesses that they accept each other as husband and wife and that they will faithfully fulfill all duties which the law connects to their marital status" (Article 67 of Book 1 of the Dutch Civil Code) and usually hold a civil ceremony and a wedding. In contrast, a registered partnership formally starts with a registration through a registrar.

Gutiérrez-Domènech, 2008). Married couples anticipating a higher probability of divorce give birth to fewer children (Becker et al., 1977; Fan, 2001; Lillard and Waite, 1993). In aging societies such as most of the developed countries and China, the constantly low or the further decline in fertility rate has been a serious economic and demographic issue.

Specifically, we exploit same-sex marriage legalization in the Netherlands as a shock to marital institution to investigate the effect of this legislation and the effect of getting married on the stability of same-sex formal partnerships already existing before the legalization. In 1998, registered partnerships were introduced in the Netherlands. This type of partnerships has been also open to same-sex couples since its initiation. Later, same-sex marriages were legalized in 2001. We focus on same-sex formal partnerships, namely registered partnerships and marriages, since (1) sexual minorities are the objects of this legal recognition, and (2) registered partnership and marriage are almost equivalent with a difference in symbolic meaning (Lee Badgett et al., 2008; Waaldijk, 2001), which enables identification of the effect of the symbol of marriage.[2] In general, same-sex marriage legalization in one's jurisdiction of residence significantly decreases the costs of marriage entry and hence increases the (net) value of marriage (Farmer and Horowitz, 2015). Rational individuals choose to enter or maintain a marriage conditional on agreement with their partner if the gains of marriage outweigh its costs, or the (net) value of marriage is positive. Thus, this legislation may impact the stability of same-sex formal partnerships in three ways. First, if one but not both partners passes the threshold in his or her utility function of marriage, one partner prefers to enter a marriage while the other partner prefers to stay in the current registered partnership. This disagreement may induce conflicts and even disruption of the registered partnership. Second, if both partners pass the marriage threshold in their utility function, they agree to transfer their current registered partnership to marriage. Third, after the transition to marriage, the symbolic significance of marriage stabilizes the existing partnership. We account for all these three channels in our study.

Using rich administrative data from Statistics Netherlands, we employ a bivariate mixed proportional hazard (MPH) model with competing risks. This allows us to investigate transitions from registered partnership to marriage simultaneously with transitions from registered partnership to divorce. In the same model we also study whether a partnership became more stable after transforming to marriage. Our method endows us with

[2]There is another minor difference between registered partnership and marriage in the procedure of bilaterally consensual divorce only which we will discuss in Section 3.2. As a sensitivity analysis, we also estimate the effect of canceling the higher costs of divorce from marriage on the stability of different-sex marriages in Section 3.5.2.

the capacity to make a distinction between two types of selectivity and "treatment". Selectivity may occur if inherently more stable relationships are more likely to transform into a marriage i.e. positive selection, or less likely to do so i.e. adverse selection. Alternatively, marriage may have a treatment effect if the transition from registered partnership to marriage renders the existing relationship more stable due to the symbolic significance of marriage. We take into account these two types of selection effects and different kinds of partnerships in stability by allowing both observables and unobservables to affect the transition into marriage and the transition into divorce simultaneously. In our model two pairs of unobserved heterogeneity capture unobservables in the transition to marriage and in the divorce hazard, respectively. We rely on the correlations of these two pairs of unobserved heterogeneity to identify the selection effects. Accounting for selectivity we can establish a causal treatment effect of getting married on the stability of same-sex relationships.[3]

We find that indeed after transforming into marriage, relationships became more stable than previous registered partnerships. The symbol of marriage has a clear stabilizing treatment effect on same-sex partnerships even after accounting for the effect of divorce costs. We also find that same-sex marriage legislation increased divorce from existing registered partnerships from before the legalization while at the same time initiating the transition to marriage. Moreover, we discover adverse selection of stable same-sex registered partnerships less inclined to transform into marriage for males but not for females. Comparing the stability of same-sex marriages and same-sex registered partnerships formed post-legalization, we find that marriages were more stable. Our results are robust against a wide range of sensitivity analyses.

We study the Netherlands because its highly tolerant attitude to same-sex marriages facilitates our identification of the effect of the symbolic significance of marriage.[4] For example, in the Eurobarometer 2015, 91% (the highest proportion among all the EU 28 countries) of the Dutch respondents agreed on the statement that "same sex marriages should be allowed throughout Europe", while the average across the 28 countries of the European Union was only 61% (European Commission, 2015). In such an open-

[3]In section 3.5, we discuss issues of identification of a causal effect such as the no-anticipation assumption in more detail.

[4]In the Netherlands, homosexual acts were decriminalized in 1811 following the integration of the country into the French empire (in France decriminalization occurred in 1791; see Waaldijk (2001)). The cross-country variation in decriminalization is huge. In England and Wales, sex between two men was illegal until 1967 when it was decriminalized for men over 21 years of age. The decriminalization referred to "in private" meaning for example that men could not have sex in a hotel. A similar decriminalization was introduced in 1980 in Scotland and in 1982 in Northern Island.

minded society and free atmosphere, same-sex couples can enter and terminate a formal partnership with much less discrimination and external pressure compared to in other countries. This kind of environment helps us to obtain a cleaner estimate of the effects of interest and might also make it possible to provide some implications for different-sex partnerships with our analysis.[5]

The economic literature of marriage (and partnership) has mainly focused on the economic incentives and benefits of marriage. Among them, only a handful of studies are in relation to same-sex partnerships. Zavodny (2008) explores whether the earning premium of married men also applies to cohabiting gay men finding that this is not the case. A similar conclusion is drawn by Booth and Frank (2008). Jepsen and Jepsen (2002) compare matching of same-sex male couples, same-sex female couples, different-sex cohabiting couples, and different-sex married couples. Positive assortative mating is found for all traits across all types of couples while this effect is stronger for non-labor-market traits than for labor-market traits, and stronger for different-sex cohabiting couples than for same-sex (cohabiting) couples. Oreffice (2011) estimates the effect of intra-household-bargaining on gay and lesbian couples' labor supply discovering a similar pattern of bargaining for same-sex (cohabiting) couples as for heterosexual cohabiting couples. Younger or richer partners in same-sex households have more bargaining power and supply less labor. This pattern is also established by Klawitter (2008). Lee Badgett et al. (2008) present an analysis based on Californian data collected among self-identified same-sex couples. A distinction is made between being single, having a partner but not cohabiting, cohabiting but without a registered (domestic) partnership, and having a registered partnership.[6] Registered partnership in California was not equivalent to marriage as it was not portable across state lines and was not recognized by the federal government. Nevertheless, the Californian Supreme Court argued that marriage differs from a registered partnership not in its legal rights and responsibilities but only in its symbolic meaning and common understanding. Lee Badgett et al. (2008) find some, albeit limited, evidence of economic determinants of partnership status. For example, income is positively correlated with the probability of having a registered partnership among men but not among women.

[5]The high tolerance and open mind for same-sex relationships in the Netherlands may generate a concern on external validity of our analysis. In less tolerant countries, difference may appear due to discrimination against sexual minorities. However, the effect of the symbol of marriage itself is unchanged. Since in this paper we are interested in the effect of the symbolic significance of marriage instead of the composite effect involving discrimination, we prefer a highly tolerant society like the Netherlands as our research context.

[6]The survey was held before in 2008 the same-sex marriage was legalized in California.

However, as the deinstitutionalization of marriage, the previously important economic functions of marriage including production, consumption and investment complementarity (Becker, 1974; Lundberg and Pollak, 2015; Pollak, 1985; Stevenson and Wolfers, 2007; Treas, 1993) have been largely impaired and replaced by other types of relationships (Cherlin, 2004). Our study adds to this economic literature of marriage by investigating the symbolic functions of marriage which have been considerably neglected. The effect of the symbolic significance of marriage is not trivial. Based on our analysis, transition to marriage significantly reduced the divorce hazard by 98% for male same-sex partnerships and 68% for female ones while canceling the costs of divorce that amount to about 750 euro (Teurlings Advocaten, 2019) increased the divorce hazard by 48% of different-sex marriages.

Previous studies on the stability of same-sex partnerships compare this stability with that of different-sex partnerships and explore the reasons for their difference (Carpenter and Gates, 2008; Kurdek, 2004; Lau, 2012; Manning et al., 2016). The lower stability of same-sex partnerships may be related to the lower degree of household specialization exhibited among same-sex couples, especially female same-sex couples, compared to different-sex couples (Aldén et al., 2015). However, this specialization gap narrows across cohorts (Giddings et al., 2014). Because different-sex and same-sex couples from recent cohorts have become similar in terms of economic incentives such as specialization (Giddings et al., 2014), our result on the effect of divorce costs on the stability of different-sex marriages may shed some light upon same-sex partnerships too. Becker (1981) notes that "homosexual unions are much less stable than heterosexual marriages" and that economic forces are responsible for this. Becker relates this to the higher search costs for homosexuals due to the "opprobrium attached to homosexuality" because of which, there is less information available making it harder to form stable relationships. Furthermore, since same-sex unions are less formalized, they dissolve at lower costs than different-sex marriages. Black et al. (2007) argue that gay men and lesbian women face different constraints from heterosexual individuals. Same-sex couples have fewer children than comparable different-sex couples (Andersson et al., 2006).[7] The costs of children for same-sex couples are higher because couples who wish to adopt a child also face non-monetary costs in terms of time and effort to overcome implicit and explicit discriminatory obstacles. The above explanations are related to discrimination against sexual minorities and pressure they are confronted with. In the highly tolerant society of the

[7]In our data, discussed in Section 3.3, this is also the case though this difference in the Netherlands is considerably smaller than that in the US. On average in the Netherlands, there are almost no children in a gay-man household, half a child in a lesbian household, and 1.5 children in a heterosexual household.

Netherlands, our inference on the effect of the symbolic significance of marriage based on a policy change in same-sex marital institution may provide common implications for marriages in general.

3.2 Institutional Background

Over the past decades, the Netherlands experienced big changes in the partnership market. Marriage has lost its dominant role while other types of relationships, including informal cohabitation and formal registered partnership, have become popular. In this section we are going to briefly present the evolution of registered partnership and same-sex marriage in the Netherlands and discuss their similarities and differences.[8]

3.2.1 Registered Partnerships

Registered partnerships were introduced on 1 January 1998 in the Netherlands. They have been open to both same-sex and different-sex couples since its initiation. Registered partners had many of the same rights and duties as married couples in for example tax, property and inheritance. A registered partnership was "almost a clone of marriage" (Waaldijk, 2001). Scherf (1999) provides information from a survey of recently concluded registered partnerships. Same-sex couples were asked whether they would have concluded a marriage if this had been a possibility with over 80% confirming that this would have been preferred. About 60% indicated that they would transfer the registered partnership to a marriage should that become possible in the future. According to Scherf (1999) a registered partnership had the same consequences as a marriage except for those with respect to children. In a marriage, the birth of a child automatically implies that both spouses are parents. In a registered partnership only the biological mother would be a parent in the eyes of the law whereas the partner would not be considered as a parent. Nevertheless, both partners could apply to the court for joint custody of a child. On 1 April 1998 the parenting law ended the privilege of married couples to adopt children. Since then, both individuals and couples in either a formal or an informal relationship regardless of their sexual orientation have been allowed to adopt a child. Thus in combination with this adoption law, the difference between marriage and registered partnership in terms of children disappeared.

From 1 April 2001 to 1 March 2009, married couples in the Netherlands were permitted

[8]For legal details we rely heavily on Waaldijk (2001).

to switch their marriage to registered partnership. This could be followed by a convenient and less costly divorce process without the need to go to court.[9] Since it was not always recognized abroad as a divorce and lacked the legal arrangements for children born in the marriage, this so called flash divorce procedure was abolished on 1 March 2009. In part of our analysis, we exploit the flash divorce to estimate the effect of divorce costs reduction – around 750 euro in total including court fees and legal fees (Government of the Netherlands, 2019; Teurlings Advocaten, 2019) – on the stability of different-sex formal partnerships. By comparing this effect with that of the symbolic significance of marriage, we throw light on the important role the latter plays in stabilizing formal partnerships.

3.2.2 Same-Sex Marriages

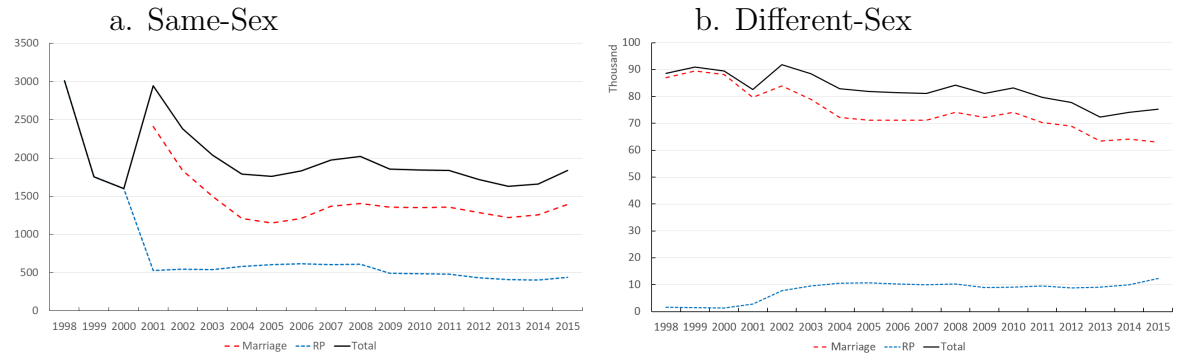
Waaldijk (2001) provides a detailed description of the characteristics of the same-sex marriage law in the Netherlands. After being approved in the Dutch parliament by the House of Representatives on 12 September 2000 and the Senate on 19 December 2000, on 1 April 2001 same-sex marriage was legalized. Since then "a marriage can be contracted by two persons of different sex or of the same sex" (Article 30 of Book 1 of the Dutch Civil Code). For the first time in human history, same-sex couples were officially and legally offered marriage equality.

Figure 3.1 shows the annual number of new registered partnerships and marriages from 1998 to 2015. Panel a displays the developments of same-sex formal relationships. Registered partnerships were popular only in the first year of their existence. After that, the number of new registered partnerships declined rapidly and then remained sort of constant from 2001 onward at the level of about 500 per year. After a spike in the first years of the same-sex marriage legalization, there were about 1400 new same-sex marriages per year.

Panel b of Figure 3.1 illustrates the evolution of new different-sex registered partnerships and marriages. The different-sex couples present a completely different pattern. In 1998, approximately 90,000 marriages were formed. Up to 2001, registered partnerships were not very popular. With the initiation of the flash divorce, the number of new reg-

[9]From a research point of view, it is inconvenient that two different changes in marriage institutions, i.e. this flash divorce and same-sex marriage legalization, were introduced on the same day. Nevertheless, we are not concerned that this threatens the identification of our main effects of interest, i.e. the effect of same-sex marriage legalization and the effect of the symbolic significance of marriage. The flash divorce focused on the administrative process of transforming a marriage to a registered partnership which was easier to dissolve. However, we study the first registered partnerships of individuals, which were not targeted by the flash divorce. Moreover, if registered partners chose to marry because of the option of flash divorce, this kind of selectivity would be captured by our MPH model.

Figure 3.1: New Marriages and Registered Partnerships; 1998-2015



Source: Statistics Netherlands

istered partnerships started to rise persisting at a level of around 10,000 per year from 2003 onward. Although initially many registered partnerships occurred because of the transition from marriage, later on new registered partnerships not immediately following a marriage became more important. After March 1st, 2009 due to the abolition of the flash divorce, new entries into registered partnerships were not preceded by a marriage.

Why would couples transfer their registered partnership to marriage? As discussed previously, there is not much difference between registered partnership and marriage in legal rights and responsibilities as well as economic incentives. A small difference between them is how the relationship legally starts and ends. People that want to marry have to declare "in the presence of the witnesses that they accept each other as husband and wife and that they will faithfully fulfill all duties which the law connects to their marital status" (Article 67 of Book 1 of the Dutch Civil Code). Registered partners formally start by registering through a registrar. A divorce of marriage can be obtained only in court. However, a registered partnership can be dissolved through a contract if both partners consent and there are no minor children involved. If either of these two conditions is not met, the divorce of a registered partnership should be dealt with in court too. Our result based on the flash divorce procedure shows that canceling divorce costs related to court and legal fees had a significant effect but could not explain the whole effect of transition to marriage on the stability of formal partnerships. What still remains is the symbolic significance attached to marriage, i.e. the public commitment and the symbol of individual prestige and personal achievement. When both marriage and registered partnership are options (after 2001 in panel a and all the years in panel b of Figure 3.1), choosing to enter a marriage rather than a registered partnership may signal a strong public commitment and personal achievement. Figure 3.1 displays that indeed much more new couples irrespective of sexual orientation preferred marriage to

registered partnership in every year.

3.3 Data

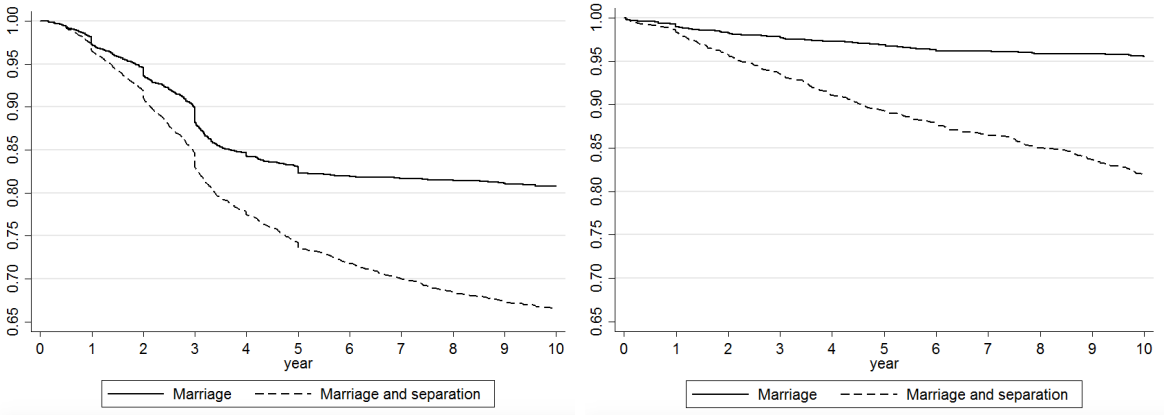
In our analysis, we use rich administrative data from Statistics Netherlands. The high quality individual level data include personal characteristics such as the country where the person was born, gender, immigrant status, birth year and month. There is also detailed information of every marital status of all individuals in the population, such as the beginning and ending dates, the birth year and month of the partner, the country where the partner was born, and the gender of the partner.

In order to make the administrative data available for our analysis we had to address a number of issues. First, we identified the sexual orientation of every person by comparing their gender with the gender of their partner in every formal partnership, i.e. registered partnership or marriage. Second, since the focus of our analysis is on the duration of formal partnerships, we established the start date of every formal relationship and when applicable the end date. We also constructed some partnership characteristics such as the age difference between the two partners and whether the couple shares the same origin, i.e. was born in the same country. Third, we gathered all, around 70,000, same-sex partnerships records. In part of our analysis, we also investigate the stability of different-sex partnerships. We randomly sampled approximately the same number of different-sex partnerships for comparison due to the highly demanding computational capacity of our model estimation. Finally, since registered partnerships were legalized in 1998, we use data about partnership formation from January 1st, 1998 onward. We follow only the first partnership of individuals starting until December 31st, 2005 to eliminate that the duration of one's later partnership was influenced by one's experiences of previous partnerships. We trace every partnership for a maximum of ten years until either their termination or the censoring time (death, widowhood, or end of data period). The reasons for such a data tailoring are that (1) partnerships with entry later than December 31st, 2005 are too far away from the year of same-sex marriage law passage, (2) these partnerships only contribute to estimates of short spells, and that (3) censoring every partnership at 10 years makes a comparison easier. The definitions and descriptives of the relevant variables in the baseline model are provided in Table 3.A.

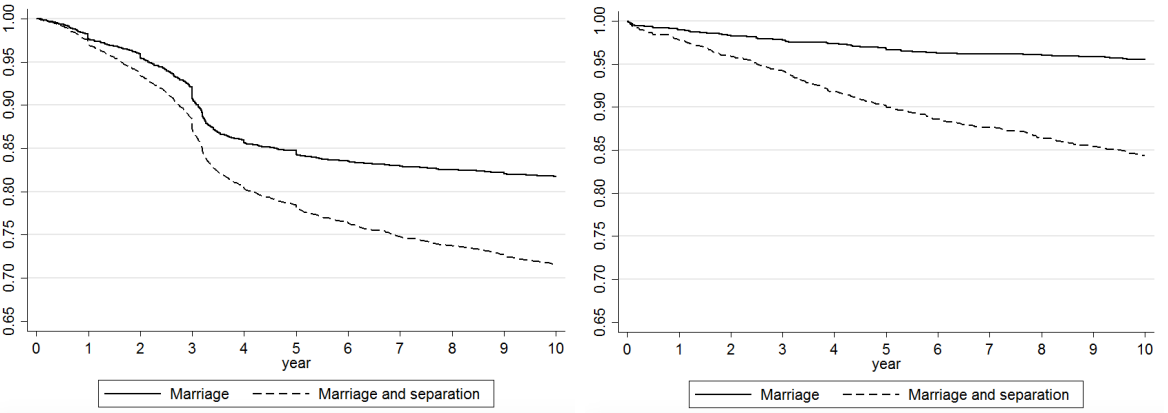
To illustrate the nature of our data, we present survival functions of same-sex registered partnerships and marriages in Figure 3.2. Panel a shows same-sex female relationships and panel b displays same-sex male relationships. In both panels the left-hand

Figure 3.2: Survival Probabilities of Same-Sex Registered Partnerships

a. Two Women; before (Left) and after (Right) the Same-Sex Marriage Law



b. Two Men; before (Left) and after (Right) the Same-Sex Marriage Law

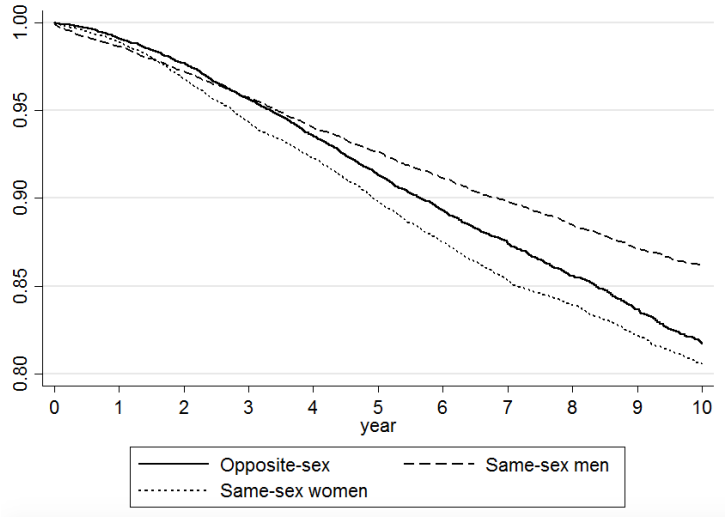


side graphs present survival functions of registered partnerships that started before the legalization of same-sex marriages. The right-hand side graphs do the same for registered partnerships that started after the legalization of same-sex marriages. Each graph indicates the transition to marriage and the cumulative transition to marriage and divorce. There is a clear difference between the left-hand side and right-hand side graphs. Registered partnerships that started before the same-sex marriage legalization are less likely to survive. After 10 years, 20 percent of the registered partnerships transferred to marriages and 10 (males) to 15 percent (females) ended in divorces. Registered partnerships that started after the same-sex marriage law are as likely to divorce but less likely to transform to marriage. This is no doubt related to the choice couples had to immediately go for a marriage rather than start with a registered partnership.

It is also interesting to compare the stability of same-sex and different-sex marriages that started after same-sex marriage legalization. As shown in Figure 3.3, during the first years of their existence, different-sex marriages had the lowest divorce risk, but later they were less stable than same-sex male marriages. Same-sex female marriages had the

lowest survival probability.

Figure 3.3: Survival Probabilities of Marriages that Started after Same-Sex Marriage Legalization



3.4 Statistical Model

We investigate the effects of same-sex marriage legalization on the stability of same-sex registered partnerships. Registered partnerships can be terminated either through dissolution or transformation to marriage. We are also interested in whether subsequent marriages are less or more stable than the preceding registered partnerships. To study both effects, we use a bivariate mixed proportional hazard approach modeling the transition processes to divorce and marriage simultaneously. Marriage entry may exert a treatment effect on the stability of same-sex registered partnerships. Therefore, we account for the possibility that after a transformation to marriage, the divorce risk changes. We start our analysis with registered partnerships that were established before the introduction of the same-sex marriage law.

We model the transition rate from a registered partnership to marriage as follows. The marriage rate at duration t conditional on a vector of observed characteristics x and unobserved characteristics ν_m is specified as

$$\theta_m(t|x,\nu_m) = \exp(x'\beta_m + \Sigma_k \mu_{mk} I_k(t) + \nu_m). \tag{2}$$

The subscript m denotes transformation to marriage. The vector x includes the absolute age difference between the partners in a couple, whether the couple shares the country of

origin, and whether the couple is native or first or second generation immigrant. Furthermore, this vector also includes birth year cohorts, age cohorts of partnership entry, and partnership entry year dummies. The term $\Sigma_k \mu_{mk} I_k(t)$ represents piece-wise constant duration dependence, i.e. duration dependence that is locally constant in connected spells or intervals. $k = 1, 2, ..., K$ is the subscript for duration interval and $I_k(t)$ are indicators which are equal to one in corresponding consecutive intervals. The true distribution of duration dependence can be arbitrarily closely approximated with a large number of duration intervals. We employ four duration intervals ($K = 4$; 0-1, 1-3, 3-7, and longer than seven years) and normalize $\mu_1 = 0$ for identification. Note that since a transition to marriage cannot take place before the legalization of same-sex marriage, the duration of transition to marriage is counted from the date of legalization.

The conditional density function of a completed registered partnership duration transitioning to a marriage can be written as:

$$f(t_m|x, \nu_m) = \theta_m(t_m|x, \nu_m) \ \exp(-\int_0^{t_m} \theta_m(s|x, \nu_m) ds) \tag{3}$$

in which t_m represents the duration of the registered partnership until it transformed to marriage. Likewise, the divorce hazard of a registered partnership at time t is as follows:

$$\theta_d(t|x, I_L, I_m, \nu_d) = \exp(x'\beta_d + \delta_L I_L + \delta_m I_m + \Sigma_k \mu_{dk} I_k(t) + \nu_d). \tag{4}$$

Similarly, the subscript d denotes divorce. The vector x contains the same observed characteristics and the pattern of duration dependence is the same as before. New elements in the specification of the divorce rate are the two indicator variables. The first is $I_L = I(t > t_L)$, which denotes whether or not the duration of the registered partnership was beyond the legalization of same-sex marriage on April 1st, 2001. Although this is a fixed calendar date, registered partnerships started at different points in calendar time. Thus different couples would pass the date of legalization at different durations of their registered partnership. Therefore, we are able to distinguish this effect from duration dependence. The parameter δ_L captures the effect of same-sex marriage legalization on the divorce hazard. A positive δ_L would indicate that the legalization raised the divorce rate possibly due to disagreement between the partners on the next phase of the relationship, i.e. marriage (Farmer and Horowitz, 2015). A negative δ_L would represent a stabilizing effect of same-sex marriage legalization. The second indicator variable in the divorce hazard is $I_m = I(t > t_m)$ which denotes whether a registered partnership was

transferred into a marriage. The parameter δ_m identifies the change in the divorce risk after getting married. A negative δ_m would imply that marriage induced relationships to become more stable while a positive δ_m would imply that marriage would be less stable than the preceding registered partnership.

The conditional density function of a completed registered partnership duration ending with a divorce can be written as:

$$f(t_d|x, I_L, I_m, \nu_d) = \theta_d(t_d|x, I_L, I_m, \nu_d) \ \exp(-\int_0^{t_d} \theta_d(s|x, I_L, I_m, \nu_d)ds) \tag{5}$$

where t_d denotes the duration of the registered partnership until it dissolved. In our analysis it is important to account for potential selectivity in the transition from registered partnership into marriage. Registered partnerships that transformed to marriage may be different for unobserved reasons from those that did not. It could be that more stable registered partnerships switched to marriage while less stable ones dissolved. If this were the case and we failed to take such a selectivity into account, we might wrongly interpret a significant negative estimate of δ_m as a treatment effect, i.e. a stabilizing effect of marriage on formal partnerships.

To disentangle the selection effect from the treatment effect of marriage, we model the transitions from registered partnerships to divorce and to marriage simultaneously and allow the two corresponding unobserved heterogeneity components ν_s and ν_m to be correlated. These two components represent common unobserved time-invariant confounding factors. A major advantage of utilizing this kind of approach is that, as shown by Abbring and Van den Berg (2003), identification of the treatment effect does not rely on a conditional independence assumption and it is not necessary to have a valid instrumental variable to establish a causal effect. Instead, identification comes from the timing of events, namely the order in which divorces and transitions to marriage occurred. To establish whether one event has a causal effect on the hazard of the other event, the key identification assumption is no-anticipation. This assumption imposes a recursive structure on the underlying process. No-anticipation does not imply that forward-looking individuals cannot have an expectation on possible future events. As long as they do not act on this expectation by changing outcomes, the no-anticipation assumption is not violated (Abbring and Van den Berg, 2003). In the context of our study, the no-anticipation assumption allows for the possibility that a couple in a registered partnership planned to marry in the future. This assumption still holds if the couple did not change the duration of their current registered partnership in response to their expectation or plan

of marriage. First, it does not make sense that couples brought forward or postponed their previously planned marriage merely because they scheduled this marriage before. Second, if a registered partnership dissolved since one partner proposed marriage while the other did not want it and hence decided to divorce, this is taken into account in our model investigating the effect of same-sex marriage legalization.[10] Identification also relies on the mixed proportional structure of the hazard rates. We use a very flexible specification of the hazard rates as we do not impose functional form assumptions on age dependence or on the distributions of unobserved heterogeneity in the hazards of marriage and divorce.

The joint conditional density function of completed durations of registered partnerships that end with either divorce or marriage can be specified as

$$f(t_m, t_d|x, I_L, I_m) = \int_{\nu_m} \int_{\nu_d} f(t_d|x, I_L, I_m, \nu_d) f(t_m|x, \nu_m) dG(\nu_d, \nu_m) \tag{6}$$

in which $G(\nu_d, \nu_m)$ is the joint discrete distribution of the two unobserved heterogeneity components each of which is supposed to take two values. Because we also estimate constants, we normalize $\nu_{d1} = \nu_{m1} = 0$. The associated probabilities are

$$\begin{aligned} p_1 &= \text{Prob}(\nu_d = \nu_{d1}, \nu_m = \nu_{m1}) \\ p_2 &= \text{Prob}(\nu_d = \nu_{d2}, \nu_m = \nu_{m1}) \\ p_3 &= \text{Prob}(\nu_d = \nu_{d1}, \nu_m = \nu_{m2}) \\ p_4 &= \text{Prob}(\nu_d = \nu_{d2}, \nu_m = \nu_{m2}) \end{aligned} \tag{7}$$

where p_j is assumed to follow a multinomial logit distribution: $p_j = \frac{\exp(\alpha_j)}{\sum_j \exp(\alpha_j)}$, in which α_j is a set of parameters for programming convenience to guarantee non-negative probabilities, for $j = 1, ..., 4$ with α_4 normalized to zero. In this MPH-structure the assumption is that the unobserved components are random effects, i.e. they are orthogonal to the explanatory variables. Since both the divorce hazard and the transition-to-marriage rate are assumed to have two types, in combination there may be four types. Modelling the selection effects, these two sets of unobserved heterogeneity are able to capture some important elements of a partnership such as preferences of couples and quality of a partnership. A combination of easy divorce and easy marriage indicates an impulsive couple that is usually in a low quality partnership, while a combination of low divorce and dif-

[10] As a sensitivity analysis to further alleviate concerns on anticipation effects, we employ a subsample in which we discard registered partnerships that were established between one quarter before and one quarter after same-sex marriage legislation. The parameter estimates are very similar.

ficult marriage refers to a cautious type. Both of these represent adverse selection. The remaining two combinations imply types with positive selection — partnerships with low divorce risk more likely transform to a marriage and unstable partnerships also have a low marriage rate. Equation (6) is used as basis for our log-likelihood function that is maximized over all parameters. We perform separate estimations for male and female same-sex couples.

3.5 Parameter Estimates

3.5.1 Duration of Same-Sex Registered Partnerships

Table 3.1 provides an overview of the main parameter estimates based on same-sex registered partnerships that were formed before the introduction of same-sex marriage law.[11] Panel a reports the results of competing risks models, while panel b displays the outcomes of single risk models that ignore the correlation between the unobserved heterogeneity components of the two transition rates.

Our main interest is twofold. First, we explore the effect of same-sex marriage legalization on the divorce hazard. Second, we want to know whether or not marriage stabilized its preceding registered partnership. Although the magnitude of the effects differ for female and male partnerships, our main findings are very similar. The same-sex marriage law increased divorce rates by 48% ($exp\{0.39\} - 1$) for female partnerships and 203% for male ones. Moreover, registered partnerships that were replaced by marriages were more stable than before. Getting married reduced the divorce hazard by 68% for female partnerships and 98% for male ones. Both effects are significantly different from zero.

We also find positive duration dependence in the divorce hazard. After their first year, registered partnerships were more likely to dissolve. In later years, the divorce rate did not change much. The transition rate from registered partnership to marriage showed a strong negative duration dependence. In the first year after same-sex marriage legalization, the marriage rate was high. In later years the transition rate was substantially smaller.

The distribution of unobserved heterogeneity according to the estimates in panel a is presented at the bottom of the table. As shown, we can identify three combinations of divorce hazards and marriage rates. There is no group which conditional on observed characteristics and duration dependence has a low divorce hazard and a high marriage

[11] All parameter estimates are presented in Appendix 3.B.

rate.[12] The main difference between females and males is that the largest group for males is the one that has low transition rates to both divorce and marriage whereas for females there is also a substantial group with a combination of low transition rate to marriage and high divorce hazard.

Table 3.1: Parameter Estimates Transition Rates of Same-Sex Registered Partnerships to Marriage and Divorce (either Directly or through Marriage as an Intermediate State)

	Two Women				Two Men			
	(1) Divorce		(2) Marriage		(3) Divorce		(4) Marriage	
a. Competing risks								
Same-sex marriage law	0.39	(0.18)**			1.11	(0.22)***		
Married	-1.13	(0.30)***			-3.72	(0.42)***		
Duration dependence								
1-3 years	1.13	(0.23)***	-0.65	(0.21)***	0.89	(0.22)***	-1.07	(0.21)***
3-7 years	1.10	(0.28)***	-0.80	(0.25)***	1.21	(0.33)***	-0.62	(0.25)**
7+ years	1.02	(0.32)***	-3.14	(0.58)***	0.87	(0.38)**	-3.24	(0.49)***
Unobserved heterogeneity								
ν	$-\infty$	(—)	-5.68	(0.18)***	-3.12	(0.40)***	-6.04	(0.14)***
α_1		-0.88	(0.22)***			-1.76	(0.05)***	
α_3		0.13	(0.43)			-3.40	(0.62)***	
-Loglikelihood		4080.7				4370.0		
b. Single risks								
Same-sex marriage law	0.31	(0.18)*			0.67	(0.21)***		
Married	-0.53	(0.16)***			-1.36	(0.27)***		
Duration dependence								
1-3 years	1.13	(0.23)***	-0.63	(0.20)***	0.97	(0.24)***	-1.00	(0.21)***
3-7 years	1.09	(0.28)***	-0.77	(0.25)***	1.18	(0.33)***	-0.57	(0.27)**
7+ years plus	1.04	(0.32)***	-3.13	(0.58)***	0.85	(0.37)**	-3.21	(0.50)***
Unobserved heterogeneity								
ν	$-\infty$	(—)	-5.67	(0.18)***	-3.89	(0.40)***	-6.09	(0.14)***
α	0.00	(0.36)	-1.68	(0.06)***	-3.04	(0.31)***	-1.94	(0.05)***
-Loglikelihood	2398.0		1683.0		2251.0		2123.5	

Note: Based on 3,147 women and 4,404 men. Other covariates are included in every model but not shown for parsimony. Standard errors in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The distributions of unobserved heterogeneity in panel a is as follows

			Transition rate to	
	Women	Men	Divorce	Marriage
p_1	0.16	0.14	High	High
p_2	–	–	Low	High
p_3	0.45	0.03	High	Low
p_4	0.39	0.83	Low	Low

Panel b of Table 3.1 shows the relevant parameter estimates if we do not take into account the correlation between the unobserved heterogeneity components of the two

[12]The associated probability of the group which conditional on observed characteristics and duration dependence has a low divorce hazard and a high marriage rate converges to zero. A low divorce rate signals a stable relationship. For these relationships there is no urgent need to transfer into a marriage. Hence for this group the marriage rate is low across the board. For relationships with a high divorce rate, there are two types: one impulsive type very likely to transform to a marriage, the other type not very likely to make this transition.

transition rates. The results for female same-sex couples are similar to those in panel a. The absolute sum of the log-likelihoods of the divorce estimates in panel b is 4081.0, which is almost identical to the minus log-likelihood of the joint estimate in panel a that has a value of 4080.7. Therefore, we cannot reject the hypothesis that there is no correlation between the unobserved heterogeneity components. For male same-sex partnerships the difference between the competing risks and single risks estimates is significant. Since the absolute sum of the log-likelihoods in panel b is 4374.5 and in panel a it is 4370.0, the value of the Likelihood Ratio test equal to 12.8, which with one degree of freedom is significantly different from zero. Apparently, selectivity is an issue for male same-sex registered partnerships. There is adverse selection, i.e. partnerships that were less likely to dissolve were also less likely to transform into a marriage, and partnerships that would have been more likely disrupted were more likely to enter a marriage.

How can we interpret our main findings? For those that made the transition to marriage, the relationship became more stable. Note that according to our estimates this is not due to the selection effect such that more stable registered partnerships transfered to marriage. It is even the other way around. Less stable male same-sex partnerships were more likely to enter a marriage. Providing that marriage and registered partnership are equivalent in legal and economic functions, we attribute this stabilizing treatment effect of marriage to its symbolic significance and higher divorce costs. In Section 3.5.2 we find that divorce costs accounted for only part of the stabilizing effect and its influence dropped dramatically and quickly.

We think that the effect of same-sex marriage legalization on the stability of same-sex partnerships may be caused by disagreement between couple on future marital arrangement. Perhaps, after the law one partner wanted to marry while the other preferred to keep the current status. The disagreement between them may have induced a dissolution of the registered partnership. As suggested by Farmer and Horowitz (2015), escalation to a more advanced relationship requires agreement. When through legalization, marriage becomes an option for same-sex couples, the costs of marriage entry drop. Thus, there are two possibilities for low quality same-sex registered partnerships. First, one but not both partners passes a threshold in his or her utility function. So, one partner prefers to enter a marriage while the other partner prefers to stay in a registered partnership. This induces conflict increasing the divorce rate, which is captured by our parameter estimate of same-sex marriage legalization. The second possibility is that both partners in a low quality registered partnership pass their marriage threshold in their utility function and hence agree to marry. However, the essence of their marriage, its inherent low quality,

determines the divorce in the future. This is reflected in the adverse selection captured by the correlated unobserved heterogeneity in our model.

Table 3.2 confirms that same-sex marriages were more stable than same-sex registered partnerships. It presents parameter estimates of the same model using a sample of the first marriages and registered partnerships that started after same-sex marriage legalization. These first partnerships are tracked until the abolition of flash divorce. We simply compare the stability of these first marriages and registered partnerships. A registered partnership was censored at the time when it transformed to marriage. As shown in the table, the divorce risk of marriages was significantly lower than that of registered partnerships (reference group). After the legalization of same-sex marriages, more stable couples decided to marry while less stable couples went for registered partnership.

Table 3.2: Parameter Estimates Divorce Rates from Same-Sex Relationships (both Registered Partnerships and Marriages) Starting after the Same-Sex Marriage Law

	(1) Two women		(2) Two men	
Marriage	-0.55	(0.13)***	-1.00	(0.16)***
Duration dependence				
1-3 years	1.33	(0.20)***	0.85	(0.20)***
3-7 years	1.56	(0.27)***	1.27	(0.26)***
7+ years	1.49	(0.57)***	0.89	(0.66)
Unobserved heterogeneity				
ν	-2.16	(0.95)**	-3.42	(0.60)***
α	-2.01	(1.57)	-1.67	(0.84)**
Observations	9,061		11,069	

Note: All covariates and constant in previous models are included in every model but not shown for parsimony. Both columns use first marriage or registered partnership of every individual that started after the law and are right censored at the flash divorce ban; standard errors in parentheses;
* p < 0.1; ** p < 0.05; *** p < 0.01

3.5.2 Costs of Divorce and Duration of Marriages

In this subsection, we explore the effect of canceling the higher divorce costs on the stability of different-sex marriages. In the highly tolerant Dutch society for sexual minorities, different-sex and same-sex couples are even more similar. The result may imply that divorce costs do not account for the whole stabilizing effect of marriage identified in the previous subsection.

From April 1st, 2001 to March 1st, 2009, both different-sex and same-sex married couples could terminate their marriage through a convenient and less costly process, the so called flash divorce. These couples first changed their marital status to registered partnership. Then they chose whether to end the registered partnership or not without

going to court. It may save up to around 750 euro by divorcing with this special procedure. We exploit the flash divorce to analyze how divorce costs affected the divorce hazards of different-sex marriages. The effect of flash divorce cannot be identified for same-sex marriages since this type of marriages did not exist before 1 April 2001. The model specification is slightly adjusted based on equation (4). The divorce rate of marriages is defined as:

$$\theta_d(t|x, I_{FD}, I_{BFD}, \nu_d) = \exp(x'\beta_d + \delta_{FD} I(t > t_{FD}) + \delta_{BFD} I(t > t_{BFD}) + \Sigma_k \mu_k I_k(t) + \nu_d) \tag{8}$$

in which the indicator variables I_{FD} and I_{BFD} denote the time of the introduction and the abolition of flash divorce. Furthermore, δ_{FD} captures the effect of flash divorce introduction and δ_{BFD} is the effect of abolishing the flash divorce. The density function of completed durations of marriages, i.e. the durations until a divorce took place can be written as:

$$f_d(t|x, I_{FD}, I_{BFD}, \nu_d) = \theta_d(t_m|x, I_{FD}, I_{BFD}, \nu_d) \ \exp(-\int_0^{t_d} \theta_d(s|x, I_{FD}, I_{BFD}, \nu_d) ds). \tag{9}$$

We remove the unobserved heterogeneity by integration

$$f_d(t|x, I_{FD}, I_{BFD}) = \int_{\nu_d} f_d(t_d|x, I_{FD}, I_{BFD}, \nu_d) dH(\nu_d) \tag{10}$$

where $H(\nu_d)$ is the distribution of unobserved heterogeneity which we assume to be discrete with two points of support following a logistic distribution.

Column (1) of Table 3.3 displays the parameter estimates of different-sex marriages that began from 1998 onward.[13] The introduction of flash divorce significantly raised the divorce hazard of different-sex married couples by 48% ($exp\{0.39\} - 1$). Quite a few different-sex couples who got married between January 1st, 1998 and April 1st, 2001, took advantage of this convenient divorce procedure to end their marriage right after this procedure was available.[14]

The flash divorce procedure was abolished on March 1st, 2009. In the first column of Table 3.3 the flash divorce ban had an insignificant effect on the divorce rate of different-sex couples, which was due to the gradual decline in divorce between the flash divorce

[13]We estimated a version with unobserved heterogeneity but both ν and α were very imprecisely estimated. Also, the LR test did not reject the version reported. The complete estimation results of Table 3.3 are reported in Appendix 3.B.

[14]In a sensitivity analysis we notice that from the second year onward the flash divorce had no significant effect on the divorce hazard.

Table 3.3: Parameter Estimates Effects of Flash Divorce on Divorce Rates from Marriages

	Different-sex Marriages				Same-sex Marriages			
	From 1998 onward		After SSM-law		Two women		Two men	
Flash divorce	0.39	(0.16)**						
Flash divorce ban	-0.01	(0.08)	-0.09	(0.09)	-0.02	(0.09)	-0.13	(0.09)
Duration dependence								
1-3 years	0.81	(0.10)***	0.67	(0.11)***	0.76	(0.09)***	0.14	(0.09)
3-7 years	1.03	(0.11)***	0.95	(0.12)***	0.86	(0.11)***	0.32	(0.12)***
7+ years	0.99	(0.12)***	1.03	(0.16)***	0.64	(0.16)***	0.32	(0.18)*
Unobserved heterogeneity								
ν			$-\infty$	(—)	-2.19	(1.36)	-2.04	(0.65)***
α			2.06	(3.88)	0.19	(1.22)	-2.07	(1.23)*
Observations	15,574		12,444		15,152		16,210	

Note: All covariates and constant in previous models are included in every model but not shown for parsimony. Column (1) shows the estimates for different-sex marriages starting from 1998 onward; columns (2) to (4) contain marriages that were set up after the introduction of same-sex marriage and flash divorce. Standard errors in parentheses; * p < 0.1; ** p < 0.05; *** p < 0.01

introduction and its abolition. The second column reports the parameter estimates for different-sex marriages that started after the introduction of the flash divorce law. Also for them, the effect of flash divorce abolition was not significantly different from zero. Just for comparison, we present the estimates for same-sex marriages too in the third and fourth columns. For these marriages, the flash divorce ban had no significant effect on the divorce rate either.

One of the issues that remain is the importance of children. Children may be a vital factor influencing a couple's decision of whether or not to divorce. However, children will not be a big issue in our study since our sample consists of the first registered partnerships only. The vast majority of the same-sex couples (81.1% for females and 98.8% for males) did not have a child living at home.[15] Still, we used information about the number of children living at home in another dataset (household administrative data) and added this to our baseline model. The main parameter estimates shown in Appendix 3.C are hardly affected.

3.6 Conclusions

Over the past decades, marriage has been deinstitutionalized in the sense that its legal and economic functions have been impaired or replaced by other types of relationships. Moreover, registered partnership and marriage have been available in many countries to

[15]In the descriptives table of Appendix 3.A, the average number of children is 0.31 in the household of lesbian women and 0.01 in that of gay men.

same-sex couples who had been excluded from this kind of legal institution. In these countries, differences between registered partnership and marriage are small or basically non-existent except for the divorce costs and symbolic significance attached to marriage, including enforceable public commitment and marker of personal achievement.

Same-sex marriage legalization is a recent phenomenon that provides an opportunity to study how the symbol of marriage affects the stability of formal partnerships. Thanks to its symbolic significance, marriage may stabilize its preceding registered partnership by enforcing its unique public commitment and marking the personal prestige of the married couple. Opening up the possibility for same-sex couples to transfer their registered partnership to marriage does not necessarily imply that this is the only possible response. It may be also that the reduced costs of marriage entry due to same-sex marriage legalization have different impacts in the utility function of two partners of the couple: one partner passes its threshold of marriage while the other does not or at least not immediately. Such disagreement could indicate that the introduction of same-sex marriages stimulated divorce from registered partnerships.

We study how the Dutch same-sex marriage legalization in 2001 affected the stability of same-sex registered partnerships which were introduced in the Netherlands in 1998. We find that same-sex marriage legalization indeed caused quite a few registered partnerships to divorce. Nevertheless, many other registered partnerships transferred into marriages. We also investigate whether marriages that were transformed from registered partnerships were more stable than they originally were. In theory, marriages could be more stable because of selectivity, i.e. the inherently more stable registered partnerships transformed into marriages while the unstable ones were not. Using a bivariate hazard rate model with marriage and divorce as competing risks and allowing marriage to directly affect the divorce rate, we find that for females selectivity is not an issue while for males there is adverse selection. Apparently, same-sex male partnerships that were less likely to dissolve were also less likely to transform into a marriage. For both females and males, we find strong and significant effects of marriage on the stability of their relationship. Once turned into a marriage, relationships were much more stable than they were before as registered partnerships. Studying the effect of the flash divorce arrangement on the duration of different-sex marriages, we conclude that the divorce costs cannot explain the whole effect of transition to marriage on the stability of formal partnerships. This is all the more surprising since the main remaining difference between registered partnerships and marriages seems to be merely symbolic. The symbolic significance of marriage has powerful stabilizing effects too on interpersonal relationships.

Appendix 3.A: Definitions and Descriptives of Variables

Table 3.A.1: Definitions of Variables

Variable	Definition
Same-sex marriage law	Dummy variable of the same-sex marriage legalization
Heterosexual	Dummy variable if classified as straight partnership
Gay man	Dummy variable if classified as gay men partnership
Lesbian	Dummy variable if classified as lesbian partnership
Age difference	Absolute age difference between a couple
Same origin	Dummy variable if both partners of a couple were born in the same country
Native	Dummy variable if both parents born in the Netherlands
First generation immigrant	Dummy variable if born abroad with at least one parent born abroad too
Second generation immigrant	Dummy variable if born in the Netherlands with at least one parent born abroad too
Year dummies of partnership entry	Dummy variables of the year when the partnership started
Duration	Proceeding duration of partnership in years
Birth year cohorts	Dummies of birth year cohorts, the larger the younger cohort
Age cohorts of partnership entry	Dummy variables for age cohorts of partnership entry
Children number	Number of children living at home
Children missing	Dummy variable if number of children is missing

Table 3.A.2: Descriptives

	Lesbian Women			Gay Men		
Variable	Mean	Min	Max	Mean	Min	Max
Age difference	5.32	0	38	7.19	0	56
Children number	0.31	0	6	0.01	0	3
Percentages						
Same origin	89.96	0	100	77.27	0	100
Natives	88.18	0	100	80.72	0	100
First generation	4.16	0	100	12.58	0	100
Second generation	7.66	0	100	6.70	0	100
Partnership years$\leq$ 1	2.19	0	100	3.13	0	100
1 <Partnership years$\leq$ 3	6.93	0	100	6.63	0	100
3 <Partnership years$\leq$ 7	10.23	0	100	10.15	0	100
Partnership years> 7	80.65	0	100	80.09	0	100
1902 < Birth year $\leq$ 1912	0.22	0	100	0.09	0	100
1912 < Birth year $\leq$ 1922	0.48	0	100	0.50	0	100
1922 < Birth year $\leq$ 1932	2.67	0	100	3.97	0	100
1932 < Birth year $\leq$ 1942	5.91	0	100	10.08	0	100
1942 < Birth year $\leq$ 1952	16.75	0	100	21.93	0	100
1952 < Birth year $\leq$ 1962	35.53	0	100	32.61	0	100
1962 < Birth year $\leq$ 1972	34.13	0	100	26.68	0	100
1972 < Birth year $\leq$ 1982	4.32	0	100	4.13	0	100
Age partnership entry $\leq$ 20	0.19	0	100	0.17	0	100
20 < Age partnership entry $\leq$ 30	13.22	0	100	11.81	0	100
30 < Age partnership entry $\leq$ 40	43.66	0	100	34.97	0	100
40 < Age partnership entry $\leq$ 50	26.06	0	100	27.18	0	100
50 < Age partnership entry $\leq$ 60	10.90	0	100	16.33	0	100
Age partnership entry > 60	5.97	0	100	9.54	0	100
Children missing	8.45	0	100	14.74	0	100

Based on 3,147 lesbian women and 4,404 gay men

Appendix 3.B: Full Parameter Estimates

Table 3.B.1 presents a full set of parameter estimates corresponding to Table 3.1 panel a. Table 3.B.2 displays a full set of parameter estimates corresponding to Table 3.2. Table 3.B.3 shows a full set of parameter estimates corresponding to Table 3.3.

Table 3.B.1: Parameter Estimates Transition Rates of Same-Sex Registered Partnerships; Competing Risks

	Two Women				Two Men			
	(1) Divorce		(2) Marriage		(3) Divorce		(4) Marriage	
Same-sex marriage law	0.39	(0.18)**			1.11	(0.22)***		
Married	-1.13	(0.30)***			-3.72	(0.42)***		
Entry in 1999	0.06	(0.13)	-0.37	(0.16)**	0.29	(0.13)**	-0.19	(0.14)
Entry in 2000	-0.02	(0.14)	-0.90	(0.22)***	0.01	(0.16)	-1.05	(0.22)***
Entry in 2001 pre-law	0.19	(0.38)	-0.33	(0.37)	0.63	(0.32)**	-0.84	(0.44)*
Duration dependence								
1-3 years	1.13	(0.23)***	-0.65	(0.21)***	0.89	(0.22)***	-1.07	(0.21)***
3-7 years	1.10	(0.28)***	-0.80	(0.25)***	1.21	(0.33)***	-0.62	(0.25)**
7+ years	1.02	(0.32)***	-3.14	(0.58)***	0.87	(0.38)**	-3.24	(0.49)***
$1942 <$ Birth year ≤ 1952	0.80	(0.78)	0.39	(0.48)	0.64	(0.58)	-0.04	(0.25)
$1952 <$ Birth year ≤ 1962	1.46	(0.83)*	-0.11	(0.57)	0.99	(0.64)	-0.20	(0.34)
$1962 <$ Birth year ≤ 1972	1.79	(0.84)**	0.67	(0.60)	1.21	(0.66)*	0.01	(0.40)
Birth year > 1972	2.36	(0.89)***	2.53	(0.68)***	1.21	(0.70)*	-0.01	(0.52)
$20 <$ Age partnership entry ≤ 30	-0.67	(0.73)	-0.86	(1.06)	-0.47	(0.65)	-1.06	(0.74)
$30 <$ Age partnership entry ≤ 40	-1.00	(0.74)	0.40	(1.08)	-1.31	(0.67)*	-1.33	(0.77)*
$40 <$ Age partnership entry ≤ 50	-1.46	(0.76)*	0.28	(1.11)	-1.89	(0.71)***	-1.31	(0.80)*
$50 <$ Age partnership entry ≤ 60	-1.88	(0.85)**	0.13	(1.15)	-2.42	(0.79)***	-1.45	(0.84)*
Age partnership entry > 60	-3.16	(1.49)**	-3.40	(1.33)**	-2.86	(1.08)***	-1.66	(0.88)*
Age difference	0.01	(0.01)	0.02	(0.01)	0.01	(0.01)	0.01	(0.01)
Same origin	-0.49	(0.18)***	-0.58	(0.24)**	-0.34	(0.16)**	-0.13	(0.17)
First generation	-0.59	(0.30)**	-1.31	(0.34)***	-0.23	(0.20)	-0.21	(0.22)
Second generation	0.27	(0.18)	-0.25	(0.23)	0.22	(0.21)	-0.17	(0.26)
Constant	-4.50	(1.15)***	1.74	(1.30)	-3.01	(0.96)***	3.87	(0.88)***
Unobserved heterogeneity								
ν	$-\infty$	(—)	-5.68	(0.18)***	-3.12	(0.40)***	-6.04	(0.14)***
α_1		-0.88	(0.22)***			-1.76	(0.05)***	
α_3		0.13	(0.43)			-3.40	(0.62)***	
-Loglikelihood		4080.7				4370.0		

Note: Based on 3,147 women and 4,404 men. Standard errors in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The distributions of unobserved heterogeneity in panel a is as follows

			Transition rate to	
	Women	Men	Divorce	Marriage
p_1	0.16	0.14	High	High
p_2	–	–	Low	High
p_3	0.45	0.03	High	Low
p_4	0.39	0.83	Low	Low

Table 3.B.2: Parameter Estimates Divorce Rates from Same-Sex Relationships (both Registered Partnerships and Marriages) Starting after Legalization of Same-Sex Marriages

	(1) Two women		(2) Two men	
Marriage	-0.55	(0.13)***	-1.00	(0.16)***
Entry in 2002	-0.01	(0.17)	-0.11	(0.22)
Entry in 2003	-0.07	(0.18)	0.22	(0.22)
Entry in 2004	-0.19	(0.20)	0.24	(0.24)
Entry in 2005	-0.11	(0.22)	-0.09	(0.27)
Entry in 2006	0.07	(0.23)	0.06	(0.30)
Entry in 2007	-0.52	(0.33)	-0.63	(0.44)
Entry in 2008-9	-0.11	(0.56)	-0.32	(0.65)
Duration dependence				
1-3 years	1.33	(0.20)***	0.85	(0.20)***
3-7 years	1.56	(0.27)***	1.27	(0.26)***
7+ years	1.49	(0.57)***	0.89	(0.66)
$1942 <$ Birth year ≤ 1952	-0.86	(1.07)	-0.43	(1.23)
$1952 <$ Birth year ≤ 1962	0.16	(1.16)	-0.20	(1.38)
$1962 <$ Birth year ≤ 1972	0.73	(1.20)	0.26	(1.42)
Birth year > 1972	0.90	(1.23)	0.42	(1.47)
$20 <$ Age partnership entry ≤ 30	-1.12	(0.61)*	-2.71	(0.62)***
$30 <$ Age partnership entry ≤ 40	-2.06	(0.72)***	-3.12	(0.67)***
$40 <$ Age partnership entry ≤ 50	-2.14	(0.77)***	-3.78	(0.74)***
$50 <$ Age partnership entry ≤ 60	-1.68	(0.90)*	-4.58	(0.99)***
Age partnership entry > 60	-4.34	(1.68)***	-4.81	(1.57)***
Age difference	0.01	(0.01)	0.03	(0.01)***
Same origin	-0.11	(0.19)	-0.88	(0.18)***
First generation	0.02	(0.24)	-0.61	(0.21)***
Second generation	0.35	(0.19)*	-0.37	(0.31)
Constant	-2.22	(1.65)	0.38	(1.61)
Unobserved heterogeneity				
ν	-2.16	(0.95)**	-3.42	(0.60)***
α	-2.01	(1.57)	-1.67	(0.84)**
Observations	9,061		11,069	

Note: Both columns use first marriage or registered partnership of every individual that started after the law and are right censored at the flash divorce ban; standard errors in parentheses;
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 3.B.3: Parameter Estimates Effects of Flash Divorce on Divorce Rates from Marriages

	Different-sex Marriages				Same-sex Marriages			
	From 1998 onward		After SSM-law		Two women		Two men	
Flash divorce	0.39	(0.16)**						
Flash divorce ban	-0.01	(0.08)	-0.09	(0.09)	-0.02	(0.09)	-0.13	(0.09)
Entry in 1999	0.09	(0.10)						
Entry in 2000	-0.00	(0.10)						
Entry in 2001 pre-FD	0.26	(0.19)						
Entry in 2001 post-FD	-0.09	(0.11)						
Entry in 2002	0.05	(0.11)	0.16	(0.11)	0.02	(0.10)	0.10	(0.10)
Entry in 2003	-0.15	(0.12)	-0.04	(0.11)	-0.08	(0.11)	0.31	(0.11)***
Entry in 2004	-0.15	(0.13)	-0.05	(0.12)	-0.09	(0.12)	0.23	(0.12)*
Entry in 2005	-0.05	(0.13)	0.09	(0.12)	-0.03	(0.12)	0.08	(0.13)
Entry in 2006	-0.13	(0.14)	0.02	(0.13)	-0.04	(0.12)	0.15	(0.13)
Entry in 2007	-0.41	(0.16)***	-0.26	(0.15)*	-0.29	(0.14)**	0.06	(0.14)
Entry in 2008	-0.15	(0.16)	0.04	(0.15)	-0.20	(0.15)	0.11	(0.15)
Entry in 2009 pre-ban	0.37	(0.29)	0.54	(0.29)*	-0.13	(0.37)	0.16	(0.38)
Entry in 2009 post-ban	0.08	(0.17)	0.27	(0.16)	-0.33	(0.16)**	-0.01	(0.18)
Entry in 2010	-0.17	(0.18)	0.05	(0.18)	-0.30	(0.17)*	0.01	(0.18)
Entry in 2011	-0.14	(0.20)	0.03	(0.19)	-0.38	(0.18)**	0.11	(0.20)
Entry in 2012	0.03	(0.22)	0.22	(0.21)	-0.38	(0.19)**	0.17	(0.22)
Entry in 2013	0.02	(0.26)	0.24	(0.25)	-0.59	(0.23)***	0.09	(0.25)
Entry in 2014	-0.51	(0.41)	-0.24	(0.39)	-0.36	(0.26)	-0.08	(0.31)
Entry in 2015	-0.18	(0.73)	-0.13	(0.73)	-1.44	(0.73)**	0.51	(0.38)
Duration dependence								
1-3 years	0.81	(0.10)***	0.67	(0.11)***	0.76	(0.09)***	0.14	(0.09)
3-7 years	1.03	(0.11)***	0.95	(0.12)***	0.86	(0.11)***	0.32	(0.12)***
7+ years	0.99	(0.12)***	1.03	(0.16)***	0.64	(0.16)***	0.32	(0.18)*
$1932 <$ Birth year ≤ 1942	-0.90	(0.24)***	-0.90	(0.35)***	-1.11	(0.44)**	-1.19	(0.23)***
$1942 <$ Birth year ≤ 1952	-0.75	(0.28)***	-0.63	(0.36)*	-1.80	(0.49)***	-1.32	(0.25)***
$1952 <$ Birth year ≤ 1962	-0.77	(0.32)**	-0.62	(0.42)	-1.69	(0.51)***	-1.40	(0.29)***
$1962 <$ Birth year ≤ 1972	-0.77	(0.34)**	-0.53	(0.44)	-1.63	(0.52)***	-1.34	(0.32)***
Birth year > 1972	-0.83	(0.36)**	-0.69	(0.46)	-1.41	(0.53)***	-1.42	(0.36)***
$20 <$ Age partnership entry ≤ 30	0.04	(0.14)	0.13	(0.18)	-1.29	(0.36)***	-1.45	(0.31)***
$30 <$ Age partnership entry ≤ 40	0.00	(0.15)	0.00	(0.19)	-1.75	(0.38)***	-1.94	(0.33)***
$40 <$ Age partnership entry ≤ 50	0.10	(0.19)	0.18	(0.24)	-2.08	(0.40)***	-2.40	(0.35)***
$50 <$ Age partnership entry ≤ 60	-0.05	(0.25)	0.02	(0.31)	-2.02	(0.41)***	-2.07	(0.38)***
Age partnership entry > 60	0.48	(0.33)	0.70	(0.41)*	-1.99	(0.49)***	-1.41	(0.42)***
Age difference	0.02	(0.00)***	0.02	(0.01)***	0.01	(0.01)**	0.02	(0.00)***
Same origin	-0.29	(0.06)***	-0.24	(0.07)***	-0.12	(0.09)	-0.31	(0.07)***
First generation	0.01	(0.06)	-0.07	(0.07)	-0.21	(0.12)*	-0.46	(0.10)***
Second generation	0.25	(0.08)***	0.23	(0.09)**	0.22	(0.09)**	0.01	(0.11)
Constant	-4.23	(0.40)***	-4.00	(0.72)***	-0.48	(0.92)	0.57	(0.86)
Unobserved heterogeneity								
ν			$-\infty$	(—)	-2.19	(1.36)	-2.04	(0.65)***
α			2.06	(3.88)	0.19	(1.22)	-2.07	(1.23)*
Observations	15,574		12,444		15,152		16,210	

Note: Column (1) shows the estimates for different-sex marriages starting from 1998 onward; columns (2) to (4) contain marriages that were set up after the introduction of same-sex marriage and flash divorce. Standard errors in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Appendix 3.C: More Parameter Estimates

Table 3.C.1 presents the main parameter estimates including number of children living at home for the baseline model in panel a of Table 3.1. The results are virtually identical.

Table 3.C.1: Transition Rates of Same-Sex Registered Partnerships to Marriage and Divorce (either Directly or through Marriage as an Intermediate State); Children Number Included

	Two Women				Two Men			
	(1) Divorce		(2) Marriage		(3) Divorce		(4) Marriage	
Competing risks								
Same-sex marriage law	0.35	(0.18)**			1.14	(0.21)***		
Married	-0.86	(0.35)**			-3.78	(0.40)***		
Duration dependence								
1-3 years	1.11	(0.23)***	-0.66	(0.21)***	0.90	(0.22)***	-1.07	(0.21)***
3-7 years	1.00	(0.28)***	-0.82	(0.25)***	1.23	(0.33)***	-0.69	(0.25)***
7+ years	0.86	(0.32)***	-3.19	(0.58)***	0.89	(0.38)**	-3.34	(0.49)***
Children number	-0.15	(0.07)**	-0.04	(0.09)	0.65	(0.17)***	0.02	(0.37)
Children missing	-0.79	(0.30)***	-4.37	(0.64)***	-3.92	(0.64)***	-4.84	(0.25)***
Unobserved heterogeneity								
ν	$-\infty$	(—)	-5.60	(0.18)***	-3.07	(0.39)***	-5.87	(0.14)***
α_1		-0.45	(0.58)			-1.61	(0.05)***	
α_3		0.76	(0.88)			-3.46	(0.67)***	
-Loglikelihood	4051.2				4264.4			

Note: Based on 3,147 women and 4,404 men. Other covariates are included in every model but not shown for parsimony. Standard errors in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The distributions of unobserved heterogeneity in panel a is as follows

			Transition rate to	
	Women	Men	Divorce	Marriage
p_1	0.17	0.16	High	High
p_2	–	–	Low	High
p_3	0.57	0.03	High	Low
p_4	0.26	0.81	Low	Low

Chapter 4

Unemployment, Immigration, and Populism: Evidence from Two Quasi-Natural Experiments in the United States

I am considerably grateful to Jan C. van Ours and Arthur van Soest for their invaluable advice and guidance on this paper. I also thank Nava Ashraf, Oriana Bandiera, Pablo Barberá, Tim Besley, Marianne P. Bitler, Dan A. Black, Roberto Bonfatti, Jan Boone, Carlos Carrillo-Tudela, Francesco Caselli, Patricio Dalton, Rafael Di Tella, Jeremiah Dittmar, Robert Dur, Eleonora Freddi, Guido Friebel, Paul Frijters, Alessandro Gavazza, Reyer Gerlagh, Maitreesh Ghatak, Ethan Ilzetzki, David Jaeger, Markus Jäntti, Sacha Kapoor, Tobias Klein, Michal Kobielarz, Peter J. Kuhn, Richard Layard, Laurence van Lent, Gilat Levy, Alan Manning, Guy Michaels, Johanna Mollerstrom, Reto Odermatt, Gerard Padró i Miquel, Steve Pischke, Erik Plug, Jan Potters, Ronny Razin, Daniel Reck, Dani Rodrik, Giorgia Romagnoli, Chris Roth, Martin Salm, Edouard Schaal, David Schindler, Dana Sisak, David Soskice, Johannes Spinnewijn, Daniel Sturm, Sigrid Suetens, Achim Voss, Stephane Wolton, Noam Yuchtman, Ruishen Zhang, Haikun Zhu, seminar participants at Erasmus University Rotterdam, LSE, Tilburg University, and conference attendants at the 2018 EALE, the 2018 Asian Meeting of the ES, the 2018 China Meeting of the ES, the 2018 ESPE, the 2018 IIPF, the 2018 IZA Summer School in Labor Economics, the 2018 National Dutch Economists Day, and the 2019 SOLE for their helpful comments and suggestions. In particular, I would like to thank the Centre for Economic Performance at LSE for its hospitality while I wrote an early draft of this paper.

Abstract

This paper examines how economic insecurity and cultural anxiety have triggered the current populism in the United States. Specifically, I exploit two quasi-natural experiments, the Great Recession and the 2014 immigration crisis, to investigate the effects of unemployment and unauthorized immigration on attitudes related to populism and populist voting in the 2016 U.S. Presidential Election. I discover that recent unemployment during the Great Recession, rather than existing unemployment from before the recession, increased the probability of attitudes forming against wealthy elites by more than 14 percentage points. Such attitudes are connected with left-wing populism. I identify perceived economic unfairness as a mechanism through which recent unemployment drove left-wing populism. However, cultural anxiety rather than economic insecurity escalated by more than 12 percentage points the probability of anti-immigration attitudes developing. These attitudes are related to right-wing populism. Furthermore, I obtain evidence that cohorts economically suffering the aftermath of the Great Recession were 42 percentage points more inclined to support left-wing populist Bernie Sanders, while cohorts residing in regions most intensely impacted by the immigration crisis were 10 percentage points more likely to vote for right-wing populist Donald Trump. My study disentangles economic insecurity from cultural anxiety and links each of them to a different type of populism.

Keywords: Populism, Unemployment, Immigration, Great Recession, Voting
JEL-codes: A13, D31, J01, J64, P16

4.1 Introduction

For a decade or more, during the Great Recession and alongside the recent immigrant influx, populism has been on the rise in many Western democracies including the U.S. (Dorn et al., 2016) and part of Europe (Colantone and Stanig, 2018b; Dustmann et al., 2017). The current populism has caused the collapse of the established party system and seen the rise of prominence of radical and populist politicians, e.g. Donald Trump and Bernie Sanders in the U.S., the Sweden Democrats in Sweden, Syriza and Golden Dawn in Greece, the National Rally (formerly known as the National Front) in France, and the Five Star Movement in Italy. What has driven the recent populist tide — economic insecurity or cultural anxiety? Is there a difference between the populism triggered by economic factors and that triggered by cultural factors? This study aims to answer these two questions by investigating how unemployment and unauthorized immigration caused the surge in populism and by examining the different dimensions of populism.

Populism is defined as an "ideology" that divides society into two antagonistic camps: virtuous people versus corrupt elites and the establishment, or virtuous people versus threatening outsiders (Canovan, 1999; Kriesi and Pappas, 2015; Laclau, 1977; Mudde, 2004; Mudde and Kaltwasser, 2017; Wiles, 1969). Populism usually appears with two compatible forms — left-wing populism and right-wing populism (Aytaç and Öniş, 2014; Kaltwasser, 2018; Mudde and Kaltwasser, 2013; Rodrik, 2018a,b). In the former, "the people" refers to the "common man" with lower income or the "poor" who cannot access power. They are perceived to be exploited by, and thus opposed to, the wealthy and powerful "elites" who control the economy and define its rules. In the latter, "the people" denotes the "nation" against outsiders, i.e. foreigners or immigrants, who are regarded as threats to the popular will (Kriesi and Pappas, 2015; Kaltwasser, 2018; Mudde and Kaltwasser, 2013; Rodrik, 2018a).

Understanding what triggers populism is important if economists and policy makers want to manage its impact. Populism may harm the established and predictable order of politics and the economy that has fostered economic growth and democratic norms (Rodrik, 2018b). Populism may also exert negative influences on economic performance by imprudently changing redistribution policy under political pressure (Alesina and Rodrik, 1994; Di Tella et al., 2017; Sachs, 1990), through the banking and credit system (Rousseau, 2016), and through distrust (Algan and Cahuc, 2010; Dustmann et al., 2017; Guiso et al., 2004; Knack and Keefer, 1997). There may exist situations where "economic populism" rather than "political populism" benefits the vast majority of the nation, such

as significant overhaul and perhaps even erosion of established economic practices and restraints during severe economic downturns (Rodrik, 2018a).

Initially, with the Great Recession[1] and the 2014 immigration crisis in the U.S. as quasi-natural experiments, I employ a difference-in-differences (DID) framework to identify the effects of unemployment and unauthorized immigration on populist attitudes. During the Great Recession, large numbers of individuals who had not previously been unemployed were laid off. They form the treatment group in my first design. The corresponding control group comprises people who were never unemployed during the period covered by my data. Thus, the Great Recession affected the individuals in the treatment group more substantially than those in the control group. Individuals in the treatment group and control group share the covariate balance — a reasonable overlap on a large range of pre-treatment characteristics (Atanasov and Black, 2016).[2] Moreover, to render units in the two groups more similar, I apply nearest neighbors propensity score matching based on pre-treatment characteristics. With retrospective employment information, I also discard people who were once unemployed during the ten years preceding 2008, so that the two groups become even more comparable. The 2014 immigration crisis principally affected the West South Central region of the U.S. in the sense that the great majority of unauthorized Central American immigrants entered the U.S. through this region. Hence, the treatment group in this second design consists of residents in the West South Central region while the control group reflects those in the rest of the U.S. As in the first design, I use propensity score matching to enhance the covariate balance between the two groups. In both designs, respondents in the treatment group and control group present parallel time trends, measured pre-treatment.

I provide evidence that recent unemployment during the Great Recession increased the probability of attitudes forming against wealthy elites by more than 14 percentage points. Such attitudes are related to left-wing populism. I find evidence that a mechanism fanning the left-wing populist attitudes was perceived economic unfairness. I do not detect that unemployment from before the recession exerted a significant influence on attitudes related to populism.[3] Ruling out economic channels of actual and perceived labor

[1]The Great Recession has been used as a shock to the labor market in multiple studies (see Algan et al. (2017), Ananyev and Guriev (2016), and Dehdari (2018)).

[2]Appendix 4.A.2 compares covariates between the treated and the controlled, measured pre-treatment, to verify the covariate balance and common support.

[3]In Appendix 4.D.2 I present a simple game theoretical model of heterogeneous socio-economic classes in a society based on Ghatak and Verdier (2017)'s set-up. In such a game, mutual fighting between low and high socio-economic classes will never be a pure equilibrium unless the economy is in adverse conditions. This model fits the empirical results I document on the Great Recession.

market competitions and perceived social security crowding out, I argue that cultural anxiety or xenophobia in the West South Central region during the 2014 immigration crisis raised the probability of a negative attitude to immigration by more than 12 percentage points. This anti-immigration attitude is connected with right-wing populism. Effects of unauthorized immigration on attitudes of left-wing populism are not found.[4]

Furthermore, I account for the interactive effects of economic and cultural concerns by examining unemployment rate, immigrant proportion, and their interactions at the regional level in every quasi-natural experiment. I find no evidence that immigration exposure was a significant multiplier of the effects of regional unemployment on attitudes related to populism. Nor do I detect that individual or regional unemployment provoked extra significant hostility to immigrants during the immigration crisis, even in the entry region of these unauthorized immigrants. As a result, I disentangle economic insecurity from cultural anxiety.

In a second step, I establish the effect of recent unemployment during the Great Recession and the effect of the 2014 immigration crisis on populist voting in the 2016 U.S. Presidential Election. Bernie Sanders was representative of left-wing populists and Donald Trump representative of right-wing populists. With a pseudo panel, I show that cohorts that had a high average of recent unemployment post-Great Recession were 42 percentage points more inclined to support Sanders while cohorts that resided in the West South Central region during the immigration crisis were 10 percentage points more likely to vote for Trump. However, cohorts suffering unemployment before the Great Recession were only more likely to vote for left-centrist Clinton.

The current study contributes to several strands of literature. Principally, it adds to the academic debate on the drivers of populism taking place between the economic insecurity perspective and the cultural backlash thesis. Only a handful of studies investigate both economic and cultural determinants simultaneously. Even fewer of them examine their interactions. Inglehart and Norris (2016) establish the association between voting for populist parties across European countries and economic and cultural characteristics. They find evidence supporting cultural backlash rather than economic insecurity. Dustmann et al. (2017) interact macroeconomic indicators with regional cultural traits. They discover that more authoritarian and traditional cultural characteristics amplify the adverse effects of economic recessions on trust in political institutions, while trust is less

[4]In Appendix 4.D.3 I present another simple game model of heterogeneous cultures and identities in a society. In this game, dominant natives as the majority will always take the action of fighting against recent immigrants regardless of economic conditions. This model is also consistent with my empirical evidence on the immigration crisis.

sensitive to economic conditions in more liberal and modern areas.

I contribute to this literature on populism in three respects. First, with individual panel data, I provide the first quasi-natural experimental evidence at the individual level for the drivers of different types of populism. Second, with separate shocks to economic insecurity and cultural backlash respectively, I disentangle the economic driver from the cultural driver and link each of them to a different type of populism. Third, compared to most studies in this literature that focus on politics only (Acemoglu et al., 2013; Algan et al., 2017; Becker et al., 2017; Colantone and Stanig, 2018a; Di Tella and Rotemberg, 2018; Foster and Frieden, 2017; Guiso et al., 2017; Hatton, 2016; Jensen et al., 2017), I utilize a richer set of measures to capture different dimensions of populism. A decrease in confidence in people who are running major companies and an increase in preferences for income redistribution by imposing higher taxes on the rich (rather than by economically assisting the poor) indicate attitudes that are against wealthy elites, and in the literature these attitudes are related to left-wing populism. The anti-immigration attitude is connected with right-wing populism in the literature. Additionally, I explore populist voting behavior in the 2016 U.S. Presidential Election.

Second, this paper is relevant to a growing literature on the effects of economic hardship on social capital, especially on trust and confidence. Ananyev and Guriev (2016) exploit the 2009 economic recession in Russia to analyze the effect of income on generalized social trust and find this effect statistically and economically significant. Algan et al. (2017), Dustmann et al. (2017), and Foster and Frieden (2017) conclude that adverse economic shocks and the resulting rise in unemployment exerted negative influences on Europeans' trust in national and EU governments. However, these studies do not distinguish between unemployment that existed before the economic downturn and new unemployment caused by the adverse economic shocks. My results show that recent unemployment during the Great Recession, rather than unemployment that existed before the Great Recession, triggered the decrease in trust or confidence in the wealthy. This original finding implies that those that were not laid off until the economic recession blamed rich elites for their unemployment. Those suffering pre-existing economic hardship may merely attribute their joblessness to their own circumstances. I verify this implication when exploring the perception of economic unfairness as a mechanism.

Third, my results are closely related to studies on preferences for redistribution. Kuziemko et al. (2015) conducted randomized survey experiments, discovering that mistrust in government explains the low support for redistribution in the U.S. However, Americans strongly preferred only one redistribution policy — the estate tax target-

ing the top 0.1% of U.S. families. This may be interpreted as a wish to prevent the self-perpetuation of extreme wealth. Because of the prevailing attitudes against wealthy elites "respondents might still support (it) if, say, the government merely burns the money it collects (from the rich)". Alesina and La Ferrara (2005) and Benabou and Ok (2001) argue that people with higher-than-expected income growth are more inclined to oppose redistribution, even when they earn below-average income and benefit from redistribution. Alesina et al. (2018) find strong political polarization in preferences for redistribution and detect that only left-wing respondents react to pessimistic intergenerational mobility perception by increasing their preferences for redistribution. Intuitively, the higher the perceived importance of effort rather than luck in determining one's income, the higher the belief in the fairness of the economy, and thus the lower the preferences for redistribution (Alesina and Angeletos, 2005; Piketty, 1995). I adopt this mechanism of perceived economic unfairness in my study. What is new in my contribution to this literature is that I combine two variables in the data to distinguish two forms of preferences for redistribution, i.e. the request for imposing higher taxes on the rich and the demand for economically assisting the poor.

Moreover, this study is part of the large literature on the impact of immigration. The attitude to immigration is studied in two traditions — political economy and socio-psychology (Hainmueller and Hopkins, 2014). The former focuses on competition over resources between immigrants and natives and explains immigration attitudes from the perspective of natives' individual self-interest. The latter perceives immigration attitudes as symbolic of group identity. In socio-psychology, contact theory states that exposure to and interaction with immigrants will produce a more tolerant and friendly attitude to immigration. Threat theory, however, alleges that natives see the arrival of immigrants as a threat to the national identity, economy, and culture. The greater the number of immigrants, the bigger the threat. I examine both channels of individual self-interest and collective identity concern, and only find evidence for the latter (see also Card et al. (2012), Sniderman et al. (2004), and Tabellini (2018)). The unauthorized immigrants from Central America did not negatively impact natives in the labor market. Nor did natives regard these immigrants as a threat to their jobs and social security. The negative attitude to immigration arose mainly from cultural and identity concerns.

Last but not least, my paper sheds light on certain problems that have awaited analysis in the expanding literature on political polarization or extreme voting. Previous studies on radical right voting have failed to identify the specificity of their results and to exclude the possibility of voting for the radical left. They are unable to distinguish between a

general pattern of extreme voting and a tendency to vote particularly for either the far right or for the far left. However, I discover that recent economic deprivation during the Great Recession was the driver only for left-wing populist voting (see also March and Mudde (2005) and Stavrakakis and Katsambekis (2014)), and that recent cultural anxiety induced support only for right-wing populism (see also Dinas et al. (2016), Kuziemko and Washington (2018), Mutz (2018), and Sekeris and Vasilakis (2016)) in the 2016 U.S. Presidential Election.

4.2 Institutional Background

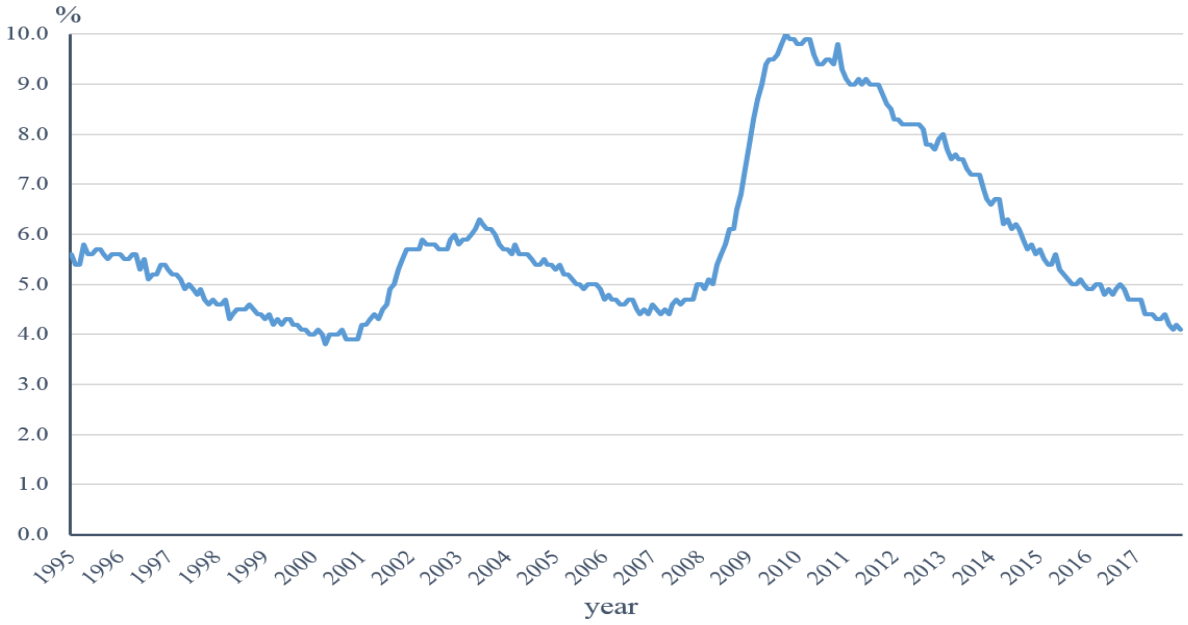
In this section I briefly discuss the evolution of the Great Recession and the 2014 immigration crisis in the U.S.

4.2.1 The Great Recession

The Great Recession has been regarded as the most influential economic recession worldwide since the Great Depression in the 1930s. It originated in 2007 with a crisis in the U.S. subprime mortgage market and spread to the banking system. Its impact was felt in financial systems around the world, with the bankruptcy of the investment bank Lehman Brothers on September 15th, 2008 as a remarkable initial signal.

Despite a variety of monetary and fiscal policies adopted by governments around the world to reduce the negative impact on the economy, the 2008 financial crisis nevertheless developed into a severe worldwide economic recession. In addition to the collapse of several banks and other financial institutions, the U.S. economy suffered a sharp drop in its output and took a serious hit on its labor market. For instance, compared to the respective previous years, U.S. real GDP decreased by around six percent at an annual rate in the last quarter of 2008 and the first quarter of 2009 (U.S. Bureau of Economic Analysis). Unemployment change is usually lagged, following GDP decline. Figure 4.1 illustrates the seasonally adjusted monthly unemployment rate in the U.S. The unemployment rate soared to over ten percent in October 2009, the highest level since 1983 and twice as high as before the Great Recession. Average working hours per week decreased to 33, the lowest since 1964 (U.S. Bureau of Labor Statistics).

Figure 4.1: Unemployment Rate in the U.S. (Seasonally Adjusted); 1995-2017



Source: U.S. Bureau of Labor Statistics

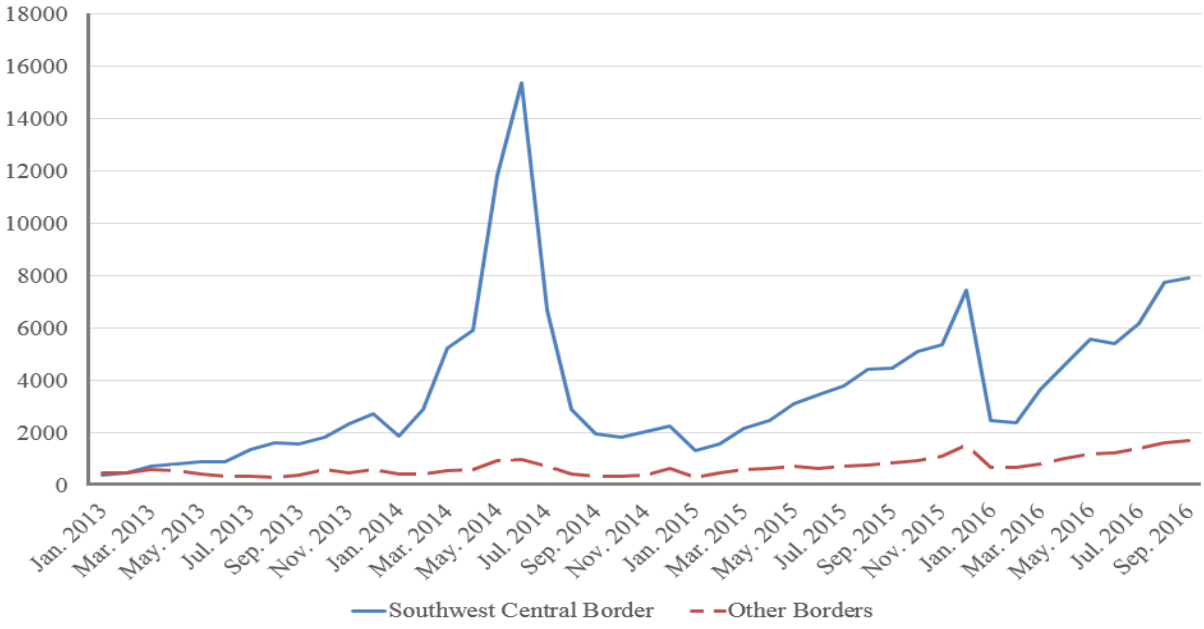
4.2.2 The 2014 Immigration Crisis

From October 2013 to late 2016, large numbers of unauthorized immigrants from the Northern Triangle of Central America, i.e. El Salvador, Guatemala, and Honduras, traveled to the U.S. southern border with Mexico, leading to an immigration crisis that peaked in 2014.[5] It turned out to be the biggest inflow of asylum seekers to the U.S. since the 1980 Mariel boatlift out of Cuba (Greenblatt, 2014). Many of them were women, unaccompanied children and juveniles. Almost all of these immigrants entered the West South Central region of the U.S., in particular from the Rio Grande Valley area located on the southern edge of Texas.[6] Figure 4.2 provides an overview of U.S. border arrests from 2013 to the third quarter of 2016: the blue line represents monthly apprehensions at the southwest central patrol sector. The inflow started to soar at the end of 2013 and reached its peak in June 2014. It then dropped dramatically at first, but started to climb again after the end of 2014. The red line denotes apprehensions at other patrol sectors in the U.S. This line is relatively flat and limited. Though this figure does not directly provide information on the number of unauthorized immigrants actually entering each month, the monthly number of arrests implies a huge variation in the influx. 55% of Northern Triangle immigrants in the U.S. were unauthorized by 2015. In 2014 the estimate of new

[5]Precise records of entries of these unauthorized immigrants are unavailable.

[6]Table 4.B.1 in Appendix 4.B lists the numbers of family unit apprehensions by month in different border patrol sectors in the U.S. from October 2012 to September 2016.

Figure 4.2: Family Unit Apprehensions by Month; 2013-Sep.2016



Source: United States Border Patrol

(authorized) immigrant arrivals from these three Central American countries is around 115,000 (Pew Research Center). Thus, if the immigrant influx in 2014 followed the same pattern as before — and in fact, during this crisis it is likely there were more unauthorized entries than authorized ones — the number of Central American immigrants entering the U.S. would be roughly doubled to 230,000 in 2014.

The principal reason that these people abandoned their family and country and took this risky and dangerous journey to the U.S. border was the mass violence in these Central American nations (U.S. Department of Homeland Security).[7] Other important drivers of this immigration crisis were the organized crime and drug trade as well as poverty and food shortage.[8]

Given the above causes, the 2014 immigration crisis has been regarded both as a

[7]DHS concluded that "(These immigrants) come from extremely violent regions where they probably perceive the risk of traveling alone to the United States preferable to remaining at home". The murder rates in these countries have skyrocketed. For example, recognized as the murder capital of the world, Honduras had a homicide rate of 91.6 murders per 100,000 people in 2011; in 2014, this number declined to 66 but was still the highest among non-war zone countries. Likewise, El Salvador also had a high rate of 90 murders per 100,000 people in 2011. Moreover, this rate dramatically increased to 104 murders per 100,000 people in 2015 after the breakdown of a truce in 2013 between the country's two most influential gangs — MS-13 and Barrio 18 (United Nations Office on Drugs and Crime).

[8]Nowadays, 79% of all cocaine-smuggled flights pass through Honduras. Children and juveniles in school are forced to smuggle drugs by criminal gangs (Nazario, 2014). Meanwhile, in the Northern Triangle area — Honduras, Guatemala, and El Salvador, most of the criminals will not be reported or prosecuted due to the lack of police force. In Guatemala, half of the children are malnourished, stunted, or even dead because of food shortage (Loewenberg, 2009).

refugee crisis and a "humanitarian crisis" (President Obama). The U.S. government took several measures in response: (1) a multimedia awareness campaign; (2) assistance to the Mexican southern border; (3) expedition of the removal process; and (4) raids in January 2016 on individuals that had exhausted their asylum claims (Hiskey et al., 2016). However, these strategies did not prove effective.[9] The Central American children and juveniles attempting to illegally cross the border are treated differently in the U.S from their Mexican counterparts. Mexican immigrants may be deported immediately, but the U.S. Trafficking Victims Protection Reauthorization Act requires that youth from Central America must be given a court hearing before they are either deported or allowed to stay. The extent of the crisis meant that in the overwhelming majority of cases, these children and juveniles would wait years for a hearing, either staying with their relatives or family friends who already lived in the U.S., or else placed in foster care (Migration Policy Institute). In fact, by the spring of 2016 most of them have not been deported (Hiskey et al., 2016). Instead, by then approximately 124,000 were immediately entitled for work authorization and free from the threat of deportation under the Deferred Action for Childhood Arrivals (DACA) program. Around 79,000 unauthorized children and juveniles from these countries had applied for the DACA program in 2016, and 65,000 of them were approved (Migration Policy Institute).

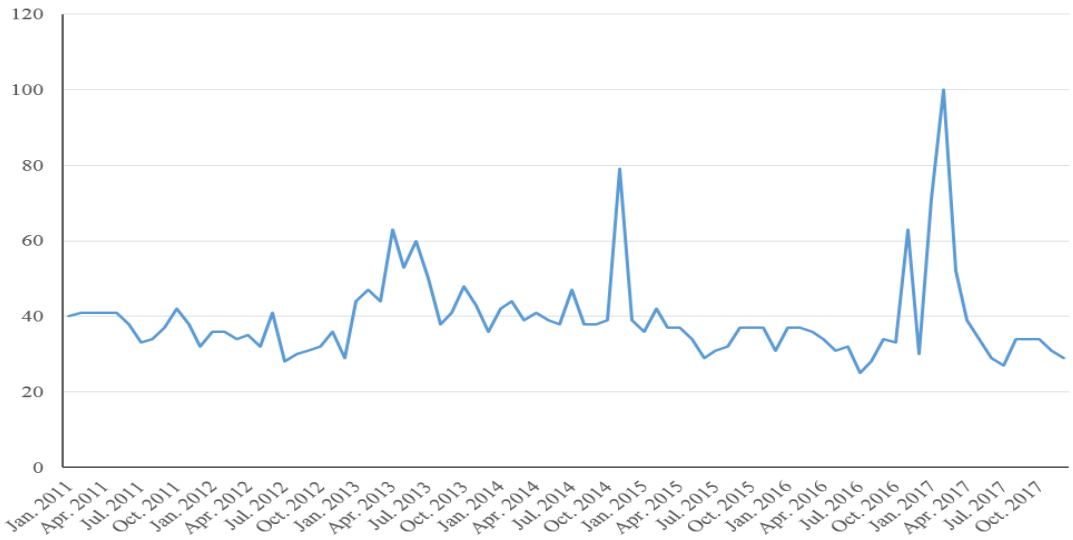
U.S. residents were aware of this immigration crisis and made their concerns known. Figure 4.3 displays the Google Trends indicator on the frequency of the term "immigration" being searched for, relative to the total number of searches in the U.S. across time. The high leap in late 2014 is remarkable, showing that U.S. citizens suddenly paid special attention to this immigrant influx.

4.3 Data

The data I utilize to analyze the economic and cultural drivers of populism are the General Social Survey (GSS) of the U.S. administered by NORC at the University of Chicago. The GSS contains a core of demographic, behavioral, and attitudinal questions. It has been conducted biennially since 1994 and has included in every wave a random sample of around 3000 (until 2004) to 4500 (since 2006) adults that is representative of the U.S. population. Hence the main body of GSS is a repeated cross-sectional dataset. However, the GSS also includes three three-wave individual panels.

[9] As the U.S. District Court Judge James Boasberg noted in his February 2015 ruling, "Defendants [DHS] have presented little empirical evidence ... that their detention policy even achieves its only desired effect, i.e., that it actually deters potential immigrants from Central America."

Figure 4.3: U.S. Google Trends on Immigration



Source: Google Trends

To obtain the voting information on the 2016 U.S. Presidential Primary Elections, I turn to the American National Election Studies (ANES) 2016 Time Series Study. This complementary dataset contains 4,271 individuals, a representative random sample of the U.S. eligible voter population. I combine it with the GSS to study populist voting.

4.3.1 Panel Data of Individuals

In addition to the repeated cross-sectional data, the GSS also includes three individual panels: the 2006-sample panel, the 2008-sample panel, and the 2010-sample panel. For example, the 2006 sample of 4,510 individuals was initially interviewed in 2006, 1,536 of them drawn randomly were re-interviewed in 2008, and then 1,276 of that 1,536 were interviewed again in 2010. The 2008- and 2010-sample panels were designed in a similar manner. There was no identifiable overlap among these three panels for me.

I exploit the 2006-sample panel to investigate the economic driver of populism and the 2010-sample panel to study the cultural driver. The former spans the pre- and post-Great Recession periods and the latter covers the pre- and post-immigration crisis phases. Even though the recent financial crisis began in the U.S. in 2008, I regard only wave 2010 as the post-Great Recession phase with respect to unemployment since unemployment rise is usually a lagged indicator of economic downturns. As shown in Figure 4.1, from 1995 to November 2008 the variation in unemployment rate displayed the same pattern at roughly the same level. Moreover, all the subjects in the year 2008 were interviewed

before October, while the bankruptcy of Lehman Brothers, signaling the beginning of this financial crisis, happened in mid-September. Therefore, it is reasonable to see the year 2008 as pre-Great Recession with respect to unemployment. The immigration crisis erupted in 2014 and lasted till 2016. Thus, in the 2010-sample panel, it is clear to classify the 2010 and 2012 waves as the pre-immigration crisis phase and the 2014 wave as post-immigration crisis. In both cases, I preserve only respondents who appeared in all three waves, resulting in two balanced panels with 1,276 individuals in the 2006-sample and 1,304 in the 2010-sample.

Based on the "ideational definition" of populism (Kaltwasser, 2018) which has been widely used in the literature in political science and political economy (Aytaç and Öniş, 2014; Kriesi and Pappas, 2015; Mudde, 2004; Mudde and Kaltwasser, 2013, 2017; Rodrik, 2018a,b), left-wing or inclusionary populist attitudes refer to attitudes against wealthy elites and the socioeconomic advantaged. In the GSS data, the variables that may most accurately capture such attitudes are confidence in people who are running major companies in the US, demand for the government to equalize the income between rich and poor, and request for the government to financially help the poor. The last two variables are two dimensions of preferences for redistribution. An increase in attitudes against wealthy elites will translate to a decrease in confidence in people running big companies, and an increase in preferences for redistribution without necessarily benefiting the poor probably by merely imposing higher taxes on the rich. Likewise, right-wing or exclusionary populist attitudes refer to attitudes against threatening outsiders such as immigrants or foreigners. Thus I adopt the attitude to immigration to represent this right-wing populist attitudes.[10] All of them are transformed so that a larger score refers to a higher level in each of these outcomes, respectively. These outcome variables can more specifically capture left-wing and right-wing populist attitudes, respectively, than variables such as trust in government or politicians in general do. Moreover, trust in government in the US is constantly low (Kuziemko et al., 2015). Its small variations across time does not help to identify the effects of economic insecurity and cultural anxiety.

The explanatory variable of interest is couple unemployment. This is constructed by combining two variables — the respondent's working status in the past week and that of their spouse if they have one.[11] Couple unemployment is a dummy that takes 1 if either partner of the couple became unemployed and takes 0 otherwise. The set of covariates

[10]Details of specific questions about the outcome variables are in Appendix 4.C.

[11]Details of specific questions about couple unemployment are in Appendix 4.C too. Alternative explanatory variables for economic insecurity, including self-unemployment of the respondent, are used for sensitivity analyses in Section 4.7.

contains the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Note that time-invariant independent variables such as gender, racial dummies, immigration status categories are canceled in the individual fixed effects model.[12]

In the DID framework, in relation to the Great Recession, the treatment group consists of respondents who became unemployed or whose spouse became unemployed, i.e. couple unemployment has a value of 1, only in wave 2010. The corresponding control group contains respondents who were not unemployed and whose spouse (if they have one) was not unemployed, that is, couple unemployment takes a value of 0, in all the three waves. The treatment group contains 81 individuals and the control group 1,109, which is comparable to the unemployment rate in the US population given that individuals having unemployment experience before the Great Recession are not included. As for the immigration crisis, the treatment group includes respondents who lived in the West South Central region of the U.S. that the immigration crisis mostly impacted in wave 2014. The precise information on these undocumented immigrant entries in different states is unavailable since they were not tracked. To be conservative, I rely on the original classification of US regions in the data (also the conventional classification of US regions) and apply the West South Central region including the states of Arkansas, Louisiana, Oklahoma, and Texas as the treated region. The corresponding control group covers those that lived in the rest of the U.S. in the same wave. The treatment group contains 136 individuals and the control group 1,168. Later in Section 4.6 and 4.7 respectively, I implement robustness checks in terms of the composition of the treated region and conduct an IV analysis on the effect of the proportion of Central American immigrants at the state level on the attitude to immigration drawing the same conclusion.

4.3.2 Pseudo Panel Data of Cohorts

To investigate the effects of the Great Recession and the 2014 immigration crisis on the populist voting in the 2016 U.S. Presidential Election, I turn to the main body of the GSS. However, with the original repeated cross-sectional data from the GSS, I am not able to link the independent variables in previous waves to voting variables in later waves or to other external voting data at the individual level. In order to address this problem,

[12]The definitions and descriptives of the relevant variables in the baseline models are provided in Appendix 4.A.1.

I construct a pseudo panel based on the repeated cross-sectional data (Deaton, 1985). Specifically, I aggregate the original data into nine ten-year birth cohorts by gender and by the nine U.S. regions where respondents resided. Hence in total there are 162 ($= 9 \times 2 \times 9$) cohorts in the sample of the pseudo panel. The average of individuals within cohort represents the corresponding cohort in every wave for every variable.

Similarly, as in Section 4.3.1, with respect to the Great Recession, the treatment group comprises cohorts whose couple unemployment averaged within cohort is greater than or equal to 0.5 in wave 2010 or 2012, and the control group contains cohorts whose couple unemployment average is less than 0.5 in both of these two waves.[13] The reason that I do not consider later waves is that the negative economic impact of the Great Recession was already extinct by 2014 and thereafter. As for the immigration crisis, the treatment and control groups are formed exactly as in the case of the individual level panel. However, I exploit information about residence in wave 2014 and 2016. All the variables are the same as in Section 4.3.1, but their values are now means within cohort in every wave.

Though there are questions about individual voting turnout and which candidate to vote for in the U.S. Presidential General Election, there is no information about the U.S. Presidential Primary Elections in the GSS survey. Since left-wing populist Bernie Sanders was a candidate only in the primaries, I need data on individual voting in the 2016 U.S. Presidential Primary Elections. These relevant questions exist in the ANES 2016 Time Series Study. I aggregate the 4,271 individuals in that dataset into cohorts in the same way as above and transform the data into a cross-section of 162 averaged cohorts. Merging this ANES 2016 cross-section with the GSS pseudo panel of cohorts, I am able to study the effects of the Great Recession and the 2014 immigration crisis on the populist voting in the 2016 U.S. Presidential Election.

4.4 Empirical Strategy

In the main estimation with the two individual level panels, I employ the ordered logit fixed effects model (Baetschmann et al., 2015) to account for time-invariant unobserved confounders. I choose this model because of the nature of ordinal dependent variables. In a sensitivity analysis, I estimate the linear fixed effects model and draw the same conclusions. The ordered logit fixed effects model in the DID setting is specified as:

[13] In Table 4.A.4.5 of Appendix 4.A.4, I also directly apply the average of couple unemployment within cohort, i.e. a continuous treatment, as the explanatory variable of interest. Though the estimates are not significant, the sign and magnitude are still as expected.

$$y^*_{it} = \beta_{Treat.\times PC} Treatment_i \times Post\_crisis_t + x'_{it}\beta_x + \alpha_i + \gamma_t + \epsilon_{it} \tag{11}$$

$$y_{it} = \begin{cases} 1, & y^*_{it} \leq c_1 \\ 2, & c_1 < y^*_{it} \leq c_2 \\ \quad ... & \\ J, & y^*_{it} > c_{J-1} \end{cases} \tag{12}$$

where i ($i = 1, 2, ..., n$) refer to individuals, and t ($t = 1, 2, ..., T$) stand for survey waves.

y represents different observed outcome variables including confidence in major companies, preferences for income redistribution, attitude to immigration, and couple unemployment (in the last two cases it is a logit fixed effects model). y^* denotes the latent counterpart of y.

$Treatment$ represents the dummy for the corresponding treatment group during the Great Recession and the 2014 immigration crisis, respectively (see Section 4.3.1). $Post\_crisis$ is either the post-Great Recession period (wave 2010) or the post-immigration crisis phase (wave 2014). Furthermore, x denotes the vector of demographic, socioeconomic, and political and ideological covariates as enumerated in the data section. In another model specification I include state-specific linear time trends to capture smooth changes in unobservables and obtain virtually identical results. Coefficients of only time-varying independent variables can be estimated in the fixed effects model where all the time-invariant variables are dropped. α_i indicate individual fixed effects and γ_t represent survey wave fixed effects. Finally, ϵ_{it} are the errors following a logistic distribution.

In order to apply the DID framework, the parallel trend assumption between the treatment and control groups should hold. This assumption in the current context implies that during the period of the last two waves, the outcome variables would follow the same trajectory between the treated and the controlled in the absence of the corresponding crisis. To assess this assumption and thus evaluate the validity of the DID setting, I examine whether the pre-shock time trends in my outcome variables diverge between the treatment and control groups. I estimate a slightly modified version of Eq.(11). Specifically, $Post\_crisis$ is replaced by the last two waves in the panels separately (with the first wave omitted as reference for identification), namely wave 2008 and wave 2010 for the Great Recession and wave 2012 and wave 2014 for the immigration crisis.[14] If the co-

[14]It is called the "Auto" model following Autor (2003) and the leads and lags model by Atanasov and Black (2016).

efficient of the interaction term of $Treatment$ and the penultimate wave is insignificantly close to zero, it is evidence for the pre-treatment parallel trends. This is indeed the case in my estimation for both the Great Recession and the immigration crisis.[15]

Moreover, in order to establish a causal link between the shocks and the outcome variables, there must not have been other events responsible for the divergence between the treatment and control groups occurring at a time close to the treatment. Placebo tests by applying fake shocks at different times during the pre-treatment period will provide such evidence if estimates are similar and statistically indistinguishable from zero before and after the fake shock (Atanasov and Black, 2016). In the current context I use only pre-treatment data. I change the onset of the fake shock to the second wave in both the Great Recession and the immigration crisis and do not detect a significant treatment effect.

To render individuals in the treatment and control groups even more comparable, two additional approaches are exploited. First, in the design of the Great Recession, I discard all the individuals who had once been unemployed in the ten years prior to 2008 in both groups. It is therefore less likely that the treated individuals and the untreated ones had different employment status during the Great Recession merely because of their divergent unobservables. Second, in designs for both the Great Recession and the 2014 immigration crisis, I adopt the method of nearest neighbors propensity score matching based on pre-shock individual characteristics. This is to improve the covariate balance between the treated and the controlled. Only the matched individuals then compose the estimation sample in the DID framework. With these two methods as robustness checks, I obtain virtually identical results.

When I study the effects of the Great Recession and the 2014 immigration crisis on the populist voting in the 2016 U.S. Presidential Election, I apply the multivariate OLS model:

$$y_i = \beta_{Treat.} Treatment_i + x_i' \beta_x + \epsilon_i. \tag{13}$$

where i denotes cohorts now. All the variables except $Treatment$ in Eq.(13) are means within cohort and thus cardinal.

y refers to expected dummy of voting for different candidates within cohort, either in

[15]Figure 4.A.3.1 illustrates the coefficient estimates of the leads and lags model for different outcome variables. None of the estimates of the interactions of $Treatment$ and the second wave is significantly distinguishable from zero. Therefore, arguably, the trends measured pre-shock between the treatment and control groups are parallel.

the 2016 U.S. Presidential Primary Elections or General Election. *Treatment* again denotes the corresponding dummy of the treatment group during either the Great Recession or the immigration crisis (see Section 4.3.2). x contains the set of means of covariates in Eq.(11) and averaged voting turnout in the corresponding elections within cohort. Guiso et al. (2017) argue that turnout incentive is vital for populist voting, so I add it to x in Eq.(13). When the dependent variable is about the primaries, x includes the voting turnout in the primaries; when the dependent variable is about the General Election, x includes the turnout in that election. As for the Great Recession, the covariates take values in wave 2010. With respect to the immigration crisis, they take values in wave 2014.

4.5 Economic Insecurity and Left-wing Populism

In this section I study the effects of economic insecurity represented by unemployment on populist attitudes. Table 4.1 displays the results based on the individual panel from 2006 to 2010.[16] The odd columns are estimates without covariates and the even columns are with controls. Robust standard errors clustered at the individual level are reported in parentheses. Only wave 2010 is the post-Great Recession period with respect to unemployment.[17]

Recent unemployment during the Great Recession may exert different effects on populism than unemployment persisting from before the Great Recession. So, I distinguish between the two types of unemployment and estimate their effects in panel a and panel b, respectively. In panel a of Table 4.1, with the DID framework, the treatment group consists of respondents who became unemployed or whose spouse became unemployed, i.e. couple unemployment has a value of 1, only in wave 2010. The corresponding control group therefore reflects a situation where neither respondent nor spouse (if they have one) was laid off, that is, couple unemployment takes a value of 0, in all the three waves.[18] In panel b, the sample excludes respondents who became unemployed or whose

[16]Table 4.A.4.1 in Appendix 4.A.4 presents the parameter estimates of the full model.

[17]To facilitate the understanding of the effect magnitudes, I report results of the linear fixed effects model in panel a of Table 4.2.

[18]In a sensitivity analysis displayed in panel a of Table 4.7, in order to make the two groups more comparable, I discard all the individuals who had lost their jobs at any time during the ten years prior to 2008. The estimates do not notably change. In another robustness check reported in Table 4.A.4.3, I include state-specific linear trends in the model and obtain virtually identical results. In the even columns of the same table, I also set wave 2008 as the onset of a placebo treatment and do not acquire any significant estimates. This therefore provides evidence that there have not been other events taking place close to the Great Recession that are responsible for the treatment effect.

Table 4.1: Effects of Unemployment on Attitudes Related to Populism

Panel a.	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Recent Unemp. Post-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat.×Post-Great Rec.	-1.23***	-1.32***	0.67**	0.70**	0.28	-0.00	-0.37	-0.28
	(0.47)	(0.50)	(0.34)	(0.34)	(0.41)	(0.43)	(0.45)	(0.48)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	1,569	1,569	5,049	5,049	3,057	3,057	878	878
Panel b.	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Existing Unemp. Pre-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Couple Unemployment	-0.61	-0.69	0.26	0.16	0.63	0.40	0.35	0.56
	(0.44)	(0.43)	(0.39)	(0.41)	(0.44)	(0.45)	(0.44)	(0.49)
Unemp.×Post-Rec.	1.96	2.02	0.35	0.48	-0.15	0.04	-1.37*	-1.33
	(1.38)	(1.53)	(0.67)	(0.73)	(0.76)	(0.77)	(0.80)	(0.90)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	1,537	1,537	5,018	5,018	3,025	3,025	912	912

Note: In panel a, the sample excludes individuals who were unemployed or whose spouse was unemployed in either wave 2006 or wave 2008. The treatment group consists of respondents who did not become unemployed or whose spouse did not become unemployed until wave 2010 and the control group contains respondents who were not laid off, and whose spouse was not laid off, at all in the three waves. Wave 2010 is the only post-Great Recession period with respect to unemployment. In panel b, the sample excludes individuals who were unemployed or whose spouse was unemployed only in wave 2010. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

spouse became unemployed only in wave 2010. The coefficients of the interaction term *Unemployment*×*post-Great Recession* represent the additive effects of couple unemployment during the Great Recession for individuals who were already unemployed before the recession.

4.5.1 Confidence in Major Companies

Lack of confidence in people managing big companies represents distrust in elites and the wealthy. Such anti-elitist attitude is connected with left-wing populism in the literature. Columns (1) and (2) of panel a show that after the Great Recession, the ordered log-odds of having a higher level of confidence in people running big companies diminished significantly by 1.3 for the recent unemployed relative to those not laid off in the data period, holding other covariates fixed. In terms of the average marginal effect, after the Great Recession the probability of having a great deal of confidence in people running major companies, that is, *trust in companies* = 3, decreased for the newly unemployed by 19 percentage points compared to the untreated individuals. The linear fixed effects

estimate displayed in Table 4.2 is around minus 0.3, sizable when compared to the corresponding adjusted mean in the treated, 0.98, over a range of zero to two. Panel b of Table 4.1 does not display significant effects on confidence in major companies among people who had already become unemployed before the Great Recession.

4.5.2 Preferences for Redistribution

Preferences for income redistribution, especially with the aim of increasing financial burden on the wealthy may be an indicator of left-wing populism. If people perceive that elites set unfair rules for the economy and take advantage of these rules to gain unfair benefits, they will ask for a higher level of redistribution. The relevant survey question inquires about preferences for redistribution by means of "raising the taxes of wealthy families or giving income assistance to the poor".

Columns (3) and (4) of panel a in Table 4.1 show the estimates of requesting the government to reduce income differences between the rich and the poor without and with controls, respectively. Ceteris paribus, if the respondent or their spouse recently became unemployed post-Great Recession, their ordered log-odds of requesting a higher level of income redistribution increased by 0.7 significantly compared to those never laid off. In other words, the probability that the newly unemployed would show the highest level of demand for redistribution, that is *government equalizes income* = 7, increased by over 14 percentage points post-Great Recession relative to the untreated.

Income equalization can be realized by raising taxes from the wealthy or by offering financial support to the poor. There is not a direct variable on taxing the rich in the survey. However, one question asks about opinions of the government's responsibility for improving the living standard of poor Americans. The results are presented in columns (5) and (6), both of which are statistically insignificant. In particular, when controlling for available covariates, the coefficient of interest becomes almost zero. In panel b, one does not observe significant estimates of preferences for redistribution.

Such an interesting discovery is noteworthy: unemployment soon after the Great Recession significantly raised demand that the government brings about income equality between rich and poor by "raising the taxes of wealthy families or giving income assistance to the poor". However, it did not increase the specific demand for the government to offer economic assistance to the poor. These two points of view are not mutually exclusive. Their preference was for the income gap between rich and poor to be reduced by imposing higher taxes on the wealthy, rather than by helping the poor financially.

Table 4.2: Linear Fixed Effects of Recent Unemployment and the Immigration Crisis on Populism

Panel a. Great Recession	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Adjusted Range	(0–2)		(0–6)		(0–4)		(0–1)	
Recent Unemp. Post-Great Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat.×Post-Great Rec.	-0.28***	-0.27***	0.47*	0.48*	0.06	-0.05	-0.03	-0.04
	(0.08)	(0.08)	(0.25)	(0.25)	(0.15)	(0.15)	(0.06)	(0.06)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	1,569	1,569	5,049	5,049	3,057	3,057	878	878
Adjusted Mean of Treat. Group	0.98		3.45		2.29		0.50	

Panel b. 2014 Immigration Crisis	All		Minorities	Caucasians	Placebo All	Placebo Cauc.	Couple Unemp.		Anticipated Unemp.	
Adjusted Range	(0–1)		(0–1)		(0–1)		(0–1)		(0–3)	
Attitude to Immigration	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat.×Post-Immig. Crisis.	-0.06	-0.06	0.06	-0.14**	-0.01	0.04	0.02	0.04	-0.08	-0.07
	(0.05)	(0.06)	(0.09)	(0.07)	(0.07)	(0.08)	(0.03)	(0.03)	(0.14)	(0.14)
Controls	No	Yes	Yes	Yes	Yes	Yes	No	Yes	No	Yes
No. of Obs.	1,070	1,070	349	690	899	635	633	633	2,848	2,848
Adjusted Mean of Treat. Group	0.52		0.52		0.52		0.08		0.68	

Note: In panel a, the sample excludes individuals who were unemployed or whose spouse was unemployed in either wave 2006 or wave 2008. The treatment group consists of respondents who did not become unemployed or whose spouse did not become unemployed until wave 2010 and the control group contains respondents who were not laid off, and whose spouse was not laid off, at all in the three waves. Wave 2010 is the only post-Great Recession period with respect to unemployment. In panel b, the treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas, that the immigration crisis most intensely impacted. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. Column (3) restricts the sample to include Hispanic Whites and non-Whites and column (4) includes non-Hispanic Whites only. Column (5) is a placebo test by using the East South Central region of the U.S. as the treated area and estimating the model excluding the West South Central region. Column (6) is the same type of placebo test for non-Hispanic Whites only. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses;
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

4.5.3 Attitude to Immigration

The last two columns in Table 4.1 examine the potential spillover of unemployment on attitude to immigration. In both panels a and b, unemployment seems to lower the positive attitude to immigration, but the coefficients are imprecisely estimated. Therefore, the hypothesis that unemployment does not affect attitude to immigration cannot be formally rejected. It is possible that when economic conditions worsen, employees in those industries more intensively exposed to immigrants may present a more negative attitude to immigration if they are laid off. In a sensitivity analysis in Section 4.7.5, I account for this industry heterogeneity in the share of immigrant workers and construct an immigrant industry dummy for either top ten or top three industries in the U.S. by share of immigrant workers. I interact it with *post-Great Recession* and with *treatment*×*post-Great Recession* and then include these two additional terms in the model. Neither of their coefficient estimates are significant. Thus, whether or not they became unemployed, workers in industries with a high proportion of immigrants did not have a more negative attitude to immigration after the Great Recession.

Nonetheless, it does not mean that economic insecurity and cultural anxiety went into effect independently. In this subsection, I show only that unemployment did not significantly change attitude to immigration. However, it is still possible that the immigrant influx caused economic insecurity in addition to cultural or identity anxiety. This may be how the economic and cultural drivers of populism interacted.

4.5.4 Mechanism

Earlier, I showed that recent unemployment during the Great Recession increased preferences for income redistribution through the imposition of higher taxes on the wealthy. How is this manifest? In the literature on preferences for redistribution, Alesina and Angeletos (2005) and Piketty (1995) allege that perceived economic unfairness generates stronger demand for income and wealth redistribution. Following their work, I use the perceived importance of effort in one's success to represent perceived economic fairness. The more important personal effort seems to be in achieving success, the fairer the economy is believed to be. And vice versa.

I explore whether perceived economic unfairness was a mechanism through which recent unemployment during the Great Recession increased preferences for redistribution. Specifically, I first examine whether new unemployment post-Great Recession created a perception that the economy was unfair. Then, I explore the effect of perceived economic

Table 4.3: Perceived Economic Unfairness: A Mechanism through which Recent Unemployment Affected Preferences for Redistribution

Panel a.	Perceived Economic Unfairness							
Recent Unemp. Post-Great Recession	(1)		(2)					
Treatment×Post-Great Recession	0.74**	(0.36)	0.77**	(0.36)				
Controls	No		Yes					
No. of Obs.	1,798		1,798					
Panel b.	Gov. Equalize Income				Gov. Help Poor			
Recent Unemp. Post-Great Recession	(1)		(2)		(3)		(4)	
Economic Unfairness	0.22*	(0.13)	0.22*	(0.13)	0.06	(0.12)	0.10	(0.12)
Controls	No		Yes		No		Yes	
No. of Obs.	2,743		2,743		1,671		1,671	
Panel c.	Perceived Economic Unfairness							
Existing Unemp. Pre-Great Recession	(1)		(2)					
Couple Unemployment	0.07	(0.44)	0.07	(0.49)				
Unemployment×Post-Great Recession	-0.25	(0.90)	-0.31	(0.82)				
Controls	No		Yes					
No. of Obs.	1,743		1,743					

Note: The samples in panels a and b exclude individuals who were unemployed in either wave 2006 or wave 2008, or whose spouse was unemployed then. The treatment group consists of respondents who, or whose spouse, did not become unemployed until wave 2010 and the control group contains respondents who were not laid off, and whose spouse was not laid off, in all the three waves. Wave 2010 is the only post-Great Recession period with respect to unemployment. In panel c, the sample excludes individuals who, or whose spouse, were unemployed in wave 2010 only. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

unfairness on preferences for redistribution. Panel a of Table 4.3 shows the results for the first step. Recent unemployment during the Great Recession did indeed increase the perception of economic unfairness, regardless of controls. In panel b, perceived economic unfairness had a significant positive effect on the demand for the government to equalize income between the wealthy and the disadvantaged. Nonetheless, this perception of unfairness did not have a significant effect on the option of assisting the poor financially. Panel c displays the first step results for those unemployed before the recession. This type of unemployment did not significantly increase the perception of economic unfairness.

The results fit with the conclusion in previous subsections: the recently unemployed during the Great Recession showed distrust in elites. They called for taxes on the wealthy to be raised, rather than for compassionate measures for the deprived. They thought that wealthy elites should be responsible for the recession and hence their economic hardship.

4.6 Cultural Anxiety and Right-wing Populism

In this section I analyze the effects of cultural anxiety on populist attitudes. An overwhelming unauthorized immigrant influx, or even an immigration crisis, may be perceived in the destination country either as an economic threat or as a cultural and identity threat. By excluding the economic channels, I argue that cultural concerns drove the anti-immigration attitude related to right-wing populism. Tables 4.4, 4.5 and 4.6 present results based on the individual panel from 2010 to 2014.[19] I apply the logit fixed effects model for *attitude to immigration* and *actual couple unemployment*, and the ordered logit fixed effects model for left-wing populist attitudes and *anticipated future unemployment*. The first column for every dependent variable does not include controls but the remaining columns do. The treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas that the immigration crisis mostly impacted.[20] The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014.

[19] Again, Table 4.A.4.2 in Appendix 4.A.4 shows the parameter estimates of the full model.

[20] In Table 4.A.4.4 of Appendix 4.A.4, I change the compositions of the treated region as robustness checks and find similar results though the estimates are less significant.

4.6.1 Attitude to Immigration

In Table 4.4 the second column with covariates reports a significant negative estimate of *treatment*×*post-immigration crisis*. In terms of the average marginal effect, for residents in the West South Central region the probability of their having a positive attitude to immigration decreased by over 12 percentage points compared to those in untreated regions. If cultural and identity concerns lowered positivity towards immigration, such an effect would be milder or even reversed among groups with a cultural background and identity that is closer to the immigrants. In columns (2I) and (2II), I estimate the same model separately for Hispanic Whites and non-Whites, and non-Hispanic Whites, respectively. Hispanic Americans are closer to these unauthorized immigrants from Central America in culture and ethnicity. Non-Whites share the identity of racial minorities with these Central American immigrants. As expected, the effect among the racial minorities (column (2I)) is nonnegative while the negative effect among non-Hispanic Whites (column (2II)) is considerably larger and significant. During the immigration crisis, for non-Hispanic Whites in the treated region the probability of a positive attitude to immigration was significantly decreased by 35 percentage points relative to those in the rest of the U.S. The corresponding linear fixed effects estimate shown in panel b of Table 4.2 is minus 0.14, and also significant. It is substantial compared to the mean attitude to immigration in the treated, 0.52. Columns (2III) and (2IV) show the estimates for bachelor's degree holders and individuals without a bachelor's degree (measured in 2014), respectively. Apparently, the lower educated group drove the anti-immigration attitude.

Column (2V) is a placebo test using the East South Central region of the U.S. (Kentucky, Tennessee, Alabama, and Mississippi) as the false treatment group. The East South Central region is similar to the West South Central region in many aspects including political inclinations. However, the 2014 immigration crisis did not notably impact the East South Central region. Thus, the East South Central region is an appropriate counterfactual. The estimate in column (2V) excludes individuals living in the West South Central region, the real treatment group. Without the remarkable influence of the 2014 immigration crisis, people in the East South Central region did not significantly change their attitude to immigration. Column (2VI) reports the estimate of the same type of placebo test among non-Hispanic Whites only, which is also statistically insignificant. Column (2VII) shows the result of another falsification test with only pre-treatment data (i.e. data of the first two waves) by using wave 2012 as a fake treatment. Column (2VIII) is the same test for the version of non-Hispanic Whites. Again, neither of them

is significant. Hence these placebo tests deliver evidence that the treatment effect is not produced by other events occurring at a time close to the immigration crisis.

In column (3) I separate the pre-immigration crisis period into wave 2010 and wave 2012 and interact them with the treatment group respectively ($treatment \times wave$ 2010 is omitted for reference). In 2012, before the immigration crisis, the difference in attitude to immigration between the treated and controlled areas did not significantly change relative to that in 2010. After Central American immigrants entered the West South Central region, the local attitude to immigration deteriorated significantly compared to the other areas.

4.6.2 Left-wing Populist Attitudes

Table 4.5 displays estimated effects of the immigration crisis on attitudes related to left-wing populism. None of the coefficients of $treatment \times post\text{-}immigration\ crisis$ is significant for confidence in big companies, the demand for the government to equalize the income between rich and poor, or the request for the government to financially assist the poor, regardless of controls inclusion. Thus I do not obtain evidence that the immigration crisis affected these left-wing populist attitudes of anti-elites.

4.6.3 Labor Market Outcomes

Natives may also see immigrants as job competitors and social welfare diggers. Table 4.6 presents results for actual unemployment of either partner of the couple and individual anticipated unemployment in the next 12 months. With or without controls, the immigration crisis resulted in an insignificant rise in actual unemployment and insignificant decline in anticipated future unemployment for people in the West South Central region. The previous subsection concludes that lower-educated natives drove the increase in the negative attitude to immigration. The reason may be the competition between immigrants and lower-educated natives in the labor market and for social welfare. Columns (2I) and (4I) include only individuals without a bachelor's degree, corresponding to column (2IV) in Table 4.4. Amazingly, the coefficient estimates are even smaller and remain insignificant.[21] Therefore, I cannot reject the hypothesis that the 2014 immigration crisis did not impact the local labor market outcomes. A similar finding has been documented by Card (2001) and Card (2005).

[21] I also investigate the effects of the immigrant influx on family income before taxes and on preferences for redistribution, and do not obtain significant estimates either. So, worries on lower wage and social security crowding out were not the reasons for the negative attitude to immigration.

Table 4.4: Effects of the Immigration Crisis on Attitude to Immigration

Attitude to Immigration							Placebo Test 1		Placebo Test 2		
	(1)	(2)	(2I)	(2II)	(2III)	(2IV)	(2V)	(2VI)	(2VII)	(2VIII)	(3)
Treat.×Post-Immig. Crisis	-0.44	-0.50*	0.15	-1.40**	-0.26	-0.62*	-0.30	-0.06	-0.31	-0.27	
	(0.28)	(0.30)	(0.50)	(0.60)	(1.09)	(0.33)	(0.47)	(0.57)	(0.40)	(0.68)	
Treat.×Wave 2012											-0.37
											(0.42)
Treat.×Wave 2014											-0.69*
											(0.37)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,070	1,070	349	690	289	781	899	635	498	318	1,070

Note: The treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas, that the immigration crisis most intensely impacted. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. Column (2I) restricts the sample to include Hispanic Whites and non-Whites and column (2II) includes non-Hispanic Whites only. Column (2III) and (2IV) show the estimates for bachelor's degree holders and individuals without a bachelor's degree (measured in 2014), respectively. Column (2V) is a placebo test by using the East South Central region of the U.S. as the treated region and estimating the model excluding the West South Central region. Column (2VI) is the same type of placebo test for non-Hispanic Whites only. Column (2VII) is another placebo test by using wave 2012 as a fake time of the shock and estimating with only pre-(real)treatment data. Column (2VIII) is the same test among non-Hispanic Whites only. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses;
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.5: Effects of Immigration Crisis on Left-wing Populist Attitudes

	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat.×Post-Immigration Crisis	0.13	0.18	-0.12	-0.11	0.09	0.01
	(0.30)	(0.32)	(0.26)	(0.28)	(0.32)	(0.34)
Controls	No	Yes	No	Yes	No	Yes
No. of Obs.	1,547	1,547	8,345	8,345	3,251	3,251

Note: The treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas, that the immigration crisis most intensely impacted. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * p < 0.1; ** p < 0.05; *** p < 0.01.

Table 4.6: Effects of the Immigration Crisis on Individual Labor Market Outcomes

	Actual Couple Unemp.			Anticipated Future Unemp.		
	(1)	(2)	(2I)	(3)	(4)	(4I)
Treatment×Post-Immigration Crisis	0.47	0.71	0.38	-0.28	-0.23	-0.17
	(0.44)	(0.45)	(0.53)	(0.33)	(0.34)	(0.37)
Controls	No	Yes	Yes	No	Yes	Yes
Number of observations	633	633	492	2,848	2,848	1,986

Note: The treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas, that the immigration crisis most intensely impacted. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. Columns (2I) and (4I) show the estimates for individuals without a bachelor's degree (measured in 2014). Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * p < 0.1; ** p < 0.05; *** p < 0.01.

Overall, I rule out the idea that economic insecurity in terms of labor market competition and social security crowding out triggered the anti-immigration attitude. Such negative attitudes prevailed much more substantially among non-Hispanic Whites, an ethnic group with cultural background and identity remote from the Central American immigrants. Thus, cultural or identity concerns are more likely to have driven the negative attitude to immigration. This result is consistent with the conclusions drawn by Card et al. (2012), Sekeris and Vasilakis (2016), and Tabellini (2018). Moreover, I do not find that the immigration crisis exerted significant influences on left-wing populist attitudes.

Therefore, economic insecurity drove left-wing populism, while cultural anxiety triggered right-wing populism.

4.7 Robustness Checks

In this section, I apply alternative methods and combine extra data to make various sensitivity analyses. The aim is to address concerns on the selection of treatment groups, the measure of individual economic insecurity, interactions of individuals' economic insecurity with regional labor market conditions and exposure to immigration, as well as the differential effects of immigration on the labor market outcomes at the regional level.

4.7.1 Propensity Score Matching

In order to improve the covariate balance between the treatment and control groups and render individuals in these two groups more comparable, I apply nearest neighbors propensity score matching.[22] More specifically, in every wave before the shock, I match the individuals in the treatment group with those in the control group based on the whole set of covariates. The propensity score is estimated with a logit model. Then I take the union of these matched individuals in different pre-shock waves to form the estimation sample for DID. Different numbers of nearest neighbors are adopted and the results are robust.

Table 4.7 displays this sensitivity analysis for the design of the Great Recession. Panel b restricts the estimation sample to include only matched treated and controlled individuals with the one-nearest neighbor matching. Panel c takes the same procedure with five-nearest neighbors matching. The results for all the outcome variables are very close to their counterparts in Table 4.1.

Likewise, panel b of Table 4.8 shows the effects of the immigration crisis on the attitude to immigration of non-Hispanic Whites with the matching method. The first two columns are for three-nearest neighbors matching and the remaining two are for five-nearest neighbors matching. The results are also similar to that in column (2II) of Table 4.4, even though the number of observations is now halved.[23] Several assumptions such as the conditional independence assumption (CIA), that potential outcomes are independent

[22]Matching methods are more appropriate for my study than the synthetic control approach because of the short pre-shock period.

[23]The estimated effects on labor market outcomes are insignificant and in similar magnitudes like before as well.

Table 4.7: Effects of Recent Unemployment on Attitudes Related to Populism: Comparability Improvement & Propensity Score Matching

Panel a.	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Recent Unemp. Post-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat.×Post-Great Rec.	-1.53**	-2.29**	1.30*	1.25*	0.78	0.40	-0.55	-0.28
	(0.77)	(1.10)	(0.67)	(0.65)	(0.70)	(0.74)	(0.52)	(0.58)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	899	899	2,717	2,717	1,638	1,638	692	692
Panel b.	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Recent Unemp. Post-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat.×Post-Great Rec.	-1.68***	-1.94***	1.04**	1.32***	0.26	0.31	-0.20	-0.16
	(0.54)	(0.60)	(0.41)	(0.45)	(0.48)	(0.50)	(0.55)	(0.81)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	303	303	1,015	1,015	613	613	171	171
Panel c.	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Recent Unemp. Post-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat.×Post-Great Rec.	-1.26***	-1.50***	0.90**	0.92***	0.27	0.11	-0.27	0.02
	(0.48)	(0.57)	(0.36)	(0.35)	(0.42)	(0.43)	(0.47)	(0.54)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	794	794	2,378	2,378	1,493	1,493	402	402

Note: In panel a, the sample excludes the individuals who were ever unemployed in the past ten years before 2008. In panel b, the sample is restricted to contain matched treated and controlled individuals only based on the one-nearest neighbor propensity score matching. In panel c, the sample is restricted to contain matched treated and controlled individuals only based on five-nearest neighbors propensity score matching. The treatment group consists of respondents who did not become unemployed or whose spouse did not become unemployed until wave 2010 and the control group contains respondents who were not laid off, and whose spouse was not laid off, at all in the three waves. Wave 2010 is the only post-Great Recession period with respect to unemployment. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.8: Effects of the Immigration Crisis on Attitude to Immigration: Industry Heterogeneity in Immigration Exposure & Propensity Score Matching

Panel a.	Attitude to Immigration			
	(1)	(2)	(3)	(4)
Treatment×Post-Immigration Crisis	-0.57*	-0.65*	-0.48*	-0.55*
	(0.31)	(0.34)	(0.29)	(0.31)
Immig. Industry×Post-Immig. Crisis	0.24	0.18	-0.31	-0.15
	(0.31)	(0.34)	(0.61)	(0.64)
Treatment×Immig. Industry×Post-Immig. Crisis	0.58	0.67	1.01	0.93
	(0.67)	(0.71)	(1.15)	(1.18)
Controls	No	Yes	No	Yes
Number of observations	1,070	1,070	1,070	1,070
Panel b.	Attitude to Immig. of Non-Hispanic Whites			
	(1)	(2)	(3)	(4)
Treatment×Post-Immigration Crisis	-1.09**	-1.92**	-1.08**	-1.42**
	(0.54)	(0.84)	(0.53)	(0.67)
Controls	No	Yes	No	Yes
Number of observations	309	309	387	387

Note: The treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas, that the immigration crisis most intensely impacted. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. In panel a, immigrant industry is a dummy for individuals who worked before 2014 in one of the industries with highest proportion of immigrant workers. In the first two columns, immigrant industry refers to the top ten industries ranked by proportion of immigrant workers; in the last two columns, it means the top three industries. In panel b, the sample is restricted to contain matched treated and controlled non-Hispanic Whites only based on nearest neighbors propensity score matching. The first two columns of panel b apply three-nearest neighbors propensity score matching and the remaining two columns apply five-nearest neighbors propensity score matching. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

of treatment conditional on observables, are necessary for the matching method. Here I show that with or without matching, the results are robust and thus the above conclusions are convincing.

4.7.2 Different Measures of New Economic Insecurity

When regarding individuals rather than couples as economic units, I also show the effects of self-unemployment of the respondents during the Great Recession on populist attitudes in the odd columns of panel a in Table 4.9. The conclusions based on these results are not changed.

Table 4.9: Effects of Recent Unemployment on Attitudes Related to Populism: Different Measures of Economic Insecurity & Interaction with Immigration Exposure

Panel a.	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Recent Unemp. Post-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat.×Post-Great Rec.	-1.49**	-0.98**	1.00***	0.68**	0.07	-0.07	-0.61	0.16
	(0.65)	(0.42)	(0.38)	(0.33)	(0.49)	(0.38)	(0.58)	(0.40)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	1,575	1,478	5,049	4,740	3,063	2,876	878	779
Panel b.	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Recent Unemp. Post-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat.×Post-Great Rec.	-1.26**	-1.22**	0.65*	0.65*	-0.02	0.02	-0.29	-0.40
	(0.51)	(0.51)	(0.34)	(0.34)	(0.44)	(0.43)	(0.48)	(0.48)
Immigrants Proportion	-0.01	-0.01	-0.07	-0.09	-0.03	-0.10*	-0.00	0.04
	(0.04)	(0.06)	(0.05)	(0.07)	(0.04)	(0.06)	(0.06)	(0.07)
Immig. Prop.×Post-Rec.	0.00	0.01	0.01	-0.01	0.01	-0.06	-0.01	-0.06
	(0.02)	(0.09)	(0.01)	(0.06)	(0.01)	(0.07)	(0.02)	(0.08)
County Unemployment		-0.03		0.05		-0.19		0.10
		(0.15)		(0.14)		(0.13)		(0.18)
County Unemp.×Post-Rec.		-0.04		-0.08		-0.03		-0.02
		(0.14)		(0.11)		(0.12)		(0.16)
Immig. Prop.×County Unemp.		-0.00		0.01		0.02**		-0.01
		(0.01)		(0.01)		(0.01)		(0.01)
Immig. Prop.×County Unemp.×Post-Rec.		-0.00		-0.00		-0.00		0.01
		(0.01)		(0.01)		(0.01)		(0.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	1,538	1,534	4,955	4,949	3,000	2,995	864	858

Note: In the odd columns of panel a, the treatment group consists of respondents who did not become unemployed until wave 2010. The control group contains respondents who were not laid off in any waves. In the even columns of panel a, the treatment group consists of respondents who, or whose spouse, did not become unemployed, or who did not anticipate very likely or fairly likely to be laid off in the next 12 months until wave 2010. The control group contains respondents who were not laid off, and whose spouse was not laid off, and who did not think they would very likely or fairly likely lose their job in the next 12 months in any waves. Wave 2010 is the only post-Great Recession period. In panel b, the treatment and control groups are constructed based on only actual individual (couple) unemployment. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Economic insecurity may contain both real and perceived dimensions. The previous treatment group in the case of the Great Recession is based on individual or couple actual unemployment only. To account for the perceived dimension of economic insecurity, I use another variable, individual anticipated unemployment in the next 12 months, in the GSS survey. If an individual answered "very likely" or "fairly likely" to the question "Thinking about the next 12 months, how likely do you think it is that you will lose your job or be laid off — very likely, fairly likely, not too likely, or not at all likely?" in wave 2010 only, they are now added to the treatment group with those recently unemployed during the Great Recession.

The corresponding results are reported in the even columns of panel a in Table 4.9. The estimates for confidence in major companies are smaller in magnitude, indicating that individual or couple actual unemployment had a stronger influence than anticipated future unemployment. The estimates for the demand for the government to equalize income are similar to those in Table 4.1. The estimates for the request for the government to provide economic assistance to the poor, and for the attitude to immigration, are again insignificant and smaller. These results fit with the previous conclusions.

4.7.3 Effects of Local Immigration Exposure and Labor Market Conditions on Left-wing Populism

If left-wing populism prevails more intensely in areas with higher proportions of immigrants, cultural or identity concerns may also contribute to it. To investigate the possible influences of cultural anxiety and its interaction with the economic shock, I include the fraction of immigrants in the state population in every wave and its interaction term with the post-Great Recession period in the model. The information on immigrants at the state level is from the American Community Survey (ACS).[24]

In panel b of Table 4.9, the odd columns report relevant results. The estimates of $treatment \times post\text{-}GreatRecession$ are still close to those in the baseline model. Moreover, neither the immigration fraction in the state population nor its interaction with post-Great Recession phase had significant effects on the outcome variables. Thus, residing in areas with more exposure to immigration did not seem to boost left-wing populist surge.

Economic insecurity may originate from one's own and one's family's economic distress, or from anxiety on the local adverse labor market conditions. Even if an individual

[24]The immigrant data at the county level are only available for part of the counties on the annual basis and hence too limited for analysis.

or their partner is not laid off, the high unemployment rate in their area may induce a movement towards left-wing populism. Hence, in the even columns of the same panel, I add the county unemployment rate and its interaction with post-Great Recession. I acquire the annual county unemployment data from the Bureau of Labor Statistics. I then include the interactions of the immigrant proportion with the county unemployment rate and with the county unemployment rate×post-Great Recession to account for the interacting effects of the economic shock and cultural concern. The estimates in the even columns show that local labor market conditions did not exert significant influences on left-wing populism, even after the Great Recession. I also do not find that immigration exposure was an important multiplier of the effects of the Great Recession on either the anti-elite attitude or the anti-immigrant attitude in regions suffering from adverse economic conditions. The coefficients of $treatment \times post\text{-}GreatRecession$ hardly change. Therefore, individual economic insecurity dominated regional economic concern in these attitudinal variations.

4.7.4 Effects of the 2014 Immigration Crisis on Local Labor Market Conditions

In Section 4.6.3, I investigate the effects of the 2014 immigration crisis on individual labor market outcomes and do not find significant impact relative to the untreated areas. Nevertheless, the effects of unauthorized immigration on the labor market may be different at the individual level than at the regional level due to changes in inflows and outflows of natives (Dustmann et al., 2017). If natives in the treated area become more negative towards immigration at the same time as the local unemployment rate is increasing, the change in attitude may be (partially) due to economic reasons. If the immigration crisis were to affect neither individual labor market outcomes nor local labor market conditions, we could be more confident that the more negative attitude to immigration was mainly driven by cultural or identity concern.

Table 4.10 reports relevant estimates. State or county fixed effects and their specific time trends, and state real GDP per capita as well as survey waves fixed effects are included. Columns (1) and (2) present the effects of the immigration crisis on the state unemployment rate and county unemployment rate, respectively. Both of them are statistically and economically insignificant (around 0.2 — 0.3 percentage points). When combined with the results in Section 4.6.3, it is clear that the immigration crisis did not impact significantly on either individual labor market outcomes or local labor market

conditions in the treated region.

Table 4.10: Effects of the Immigration Crisis on Local Labor Market Conditions and Immigrants Proportions

	Unemploy. Rate (%)		Central American Immig. Prop. (%)	
	(1)	(2)	(3)	(4)
	State	County	State	County
Treatment×Post-Immigration Crisis	0.27	0.17	0.08*	0.08***
	(0.43)	(0.20)	(0.05)	(0.02)
Controls	Yes	Yes	Yes	Yes
Number of observations	127	692	91	625

Note: The treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas, that the immigration crisis most intensely impacted. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. State or county fixed effects and survey wave fixed effects are included in every column. Controls contain state real GDP per capital and state- or county-specific time trends. Robust standard errors clustered at the state or county level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Columns (3) and (4) report the effects of the immigration crisis on the fraction of (authorized) Central American immigrants in the state of residence.[25] The covariates are the same as in the first two columns. The immigration crisis increased the proportion of (authorized) Central American immigrants in the state of residence by around 0.1 percentage points in the treated region compared to the rest of the U.S.[26] In 2015, most of the Central American immigrants living in the U.S. were unauthorized (Pew Research Center). Thus, if the unauthorized immigration inflow followed the same pattern, the effect would be roughly doubled to 0.2 percentage points. Compared to this proportion in the median state in the sample, 0.85%, the effect of the immigration crisis would not be seen as trivial.

4.7.5 Industry Heterogeneity in Immigration Exposure

Even though the immigration crisis did not negatively affect local labor market outcomes in the treated region, it is still possible that workers in industries that were more intensively exposed to immigrants would feel threatened and thus hold a more negative attitude to immigration after the immigration crisis. If so, the deterioration in attitude to immigration should be attributed to economic insecurity rather than to cultural concern.

In panel a of Table 4.8, the industry heterogeneity in the share of immigrant workers is taken into account. The first two columns set out the top ten industries ranked by share

[25] Again, the available immigrant data at the county level from ACS are too limited on a yearly basis.

[26] The estimates of numbers of unauthorized Central American immigrants by state are imprecise and not on an annual basis. Therefore, I can only use data about authorized immigrants.

of immigrant workers, namely private households (45%), textile, apparel, leather manufacturing (36%), agriculture (33%), accommodation (32%), food manufacturing (29%), computer and electronic products manufacturing (27%), personal and laundry services (26%), administrative and support services (25%), construction (24%), and miscellaneous and not specified manufacturing (23%) (Pew Research Center). The remaining two columns define immigrant industry in terms of the top three industries. More specifically, the *immigrant industry* is a time-invariant dummy of individuals who worked in one of the immigrant industries before the immigration crisis. Additional interaction terms of *immigrant industry* and *post-immigration crisis*, and of these and *treatment* region are included. Neither of the coefficients of these interactions are significant, so working in an industry with a high share of immigrant workers did not entail a more negative attitude to immigration after the immigration crisis, whether the respondent lived in the treated region or not. The estimate of *treatment*×*post-immigration crisis* remains similar to that in Table 4.4. In this way, I rule out potential economic insecurity as an explanation for the increased negative attitude to immigration in immigrant industries after the immigration crisis.

4.7.6 Effects of Individual Labor Market Outcomes and Local Labor Market Conditions on Right-wing Populism

I have not found that the 2014 Immigration Crisis exerted a significant influence on either individual labor market outcomes or local labor market conditions in the treated region. This rules out the idea that residents in the most impacted area became increasingly negative towards immigration because of any realistic adverse labor market consequences created by the unauthorized immigration. However, it does not exclude the possibility that natives facing economic hardship or living in places with adverse economic conditions scapegoat immigrants.

In Table 4.11 I add to the model a dummy for couple unemployment, the unemployment rate in county of residence, and their interactions with the post-immigration crisis. The first three columns show the attitude to immigration of the whole sample, and the last three that of non-Hispanic Whites only. The estimates for *treatment*×*post-immigrationcrisis* stay close to those in the baseline model in Table 4.4. Individual (couple) labor market outcomes represented by couple unemployment did not have significant effects on the attitude to immigration in any specifications, and this is in line with the conclusion in Section 4.5.3. While local labor market conditions captured by the county

Table 4.11: Effects of the Immigration Crisis on Attitude to Immigration: Individual Labor Market Outcomes and Local Labor Market Conditions as Additional Explanatory Variables

Attitude to Immigration		All			Non-Hispanic Whites	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat.×Post-Immig. Crisis	-0.50*	-0.43	-0.43	-1.38**	-1.32**	-1.30**
	(0.30)	(0.31)	(0.31)	(0.61)	(0.59)	(0.61)
Couple Unemployment	-0.51		-0.51	-0.67		-0.68
	(0.39)		(0.40)	(0.56)		(0.57)
Couple Unemp.×Post-Immig. Crisis	0.49		0.52	0.62		0.63
	(0.57)		(0.56)	(0.88)		(0.86)
County Unemployment		-0.09	-0.09		-0.17*	-0.16
		(0.08)	(0.08)		(0.10)	(0.10)
County Unemp.×Post-Immig. Crisis		-0.04	-0.05		-0.17	-0.18*
		(0.08)	(0.08)		(0.11)	(0.11)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.		1,070			690	

Note: The treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas, that the immigration crisis most intensely impacted. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

unemployment rate bear some marginally significant effects only among non-Hispanic Whites, we should be cautious with these estimates: first, they are merely marginally significant at the 10% level; second, in the alternative linear specification even this 10% level of significance disappears; finally, without an exogenous shock to the county unemployment rate, these estimates present correlation rather than causation. Actually, by exploiting the Great Recession as a shock to the county unemployment, I use the same model as column (8) of panel b in Table 4.9 to estimate among non-Hispanic Whites only and do not find significant coefficients for county unemployment related variables. Therefore, I do not regard either individual labor market outcomes or local labor market conditions as important drivers of the attitude to immigration connected with right-wing populism.[27]

[27]Based on columns (1) and (4) of Table 4.11, I further interact the treated region with couple unemployment and with couple unemployment×post-immigration crisis simultaneously. Neither of them has significant negative effects on attitude to immigration.

4.7.7 A Different Design for the 2014 Immigration Crisis

Previously the treatment group with respect to the 2014 immigration crisis has been the West South Central region. The unauthorized Central American immigrants entered the U.S. through this region and hence impacted there mostly intensely in the short term. However, if these immigrants moved to other areas of the U.S. within a few months, their destinations might form an appropriate treatment group. According to the Migration Policy Institute, around 90% of the children and juveniles among these immigrants later stayed with relatives or family friends who were already living in the U.S. Central American immigrants are already distributed rather unevenly in the U.S., gathering in several states and metropolitan areas (Migration Policy Institute). So the states and areas with higher proportions of Central American immigrants were more likely to be destinations for the unauthorized immigrants in 2014.

Nonetheless, the proportion of Central American immigrants by state is endogenous to the attitude of natives to immigration. There may be a reverse causality in that immigrants move to and concentrate in areas where they are more welcome or tolerated by natives. A potential empirical strategy for addressing this problem is to utilize the distance from the destination to the Rio Grande Valley border patrol sector as an instrument variable (IV) for the proportion of Central American immigrants. Note that a dominant proportion of these unauthorized immigrants (over 80% in the peak of the 2014 immigration crisis) entered the U.S. via this valley. This strategy relies on the exclusion restriction that the distance to the border patrol sector affected natives' attitude to immigration only through the proportion of Central American immigrants.[28] The use of distance as an IV in a similar context can be seen in Dinas et al. (2016).

Table 4.12 displays the 2SLS estimates. The distance is measured as the shortest driving distance from the county of residence to the Rio Grande Valley border patrol sector.[29] In the first stage, the natural logarithm of distance to the border sector is negatively associated with the proportion of (authorized) Central American immigrants in the state of residence, although this association is only statistically significant for the sample of non-Hispanic Whites. In the second stage, the proportion of Central American immigrants is positively correlated with natives' attitude to immigration. I am reluctant to interpret this effect as causal since the distance to the Rio Grande Valley sector is probably effective as an IV only for the 2014 immigration crisis. After all, Central American immigrants could previously enter the U.S. using many routes scattered along its

[28] I also try using the proportion of Latin American immigrants and obtain similar results.

[29] Another measure — distance "as the crow flies", the most direct path — yields similar results.

Table 4.12: 2SLS Estimates Effects of the Immigration Crisis on Attitude to Immigration: A Different Design of Treatment

Attitude to Immigration	All		Non-Hispanic Whites	
	(1)	(2)	(3)	(4)
	First Stage	Second Stage	First Stage	Second Stage
Ln Distance	-0.34		-0.79***	
	(0.23)		(0.17)	
Instrumented Centr. Amer. Immig. Prop.		1.02		3.06*
		(2.73)		(1.65)
Instru. Centr. Amer. Immig. Prop.×Post-Crisis		-0.56		-0.81*
		(0.45)		(0.44)
County Unemployment	0.02***	-0.12	0.02**	-0.22**
	(0.01)	(0.09)	(0.01)	(0.10)
County Unemp.×Post-Crisis	-0.00	-0.03	-0.00	-0.15
	(0.00)	(0.08)	(0.01)	(0.11)
Controls	Yes	Yes	Yes	Yes
No. of Obs.	1,070		690	

Note: The distance is measured as the shortest driving distance from the county of residence to the Rio Grande Valley border patrol sector. This distance is the instrument variable for the proportion of Central American immigrants in the state population. Wave 2014 is the only post-immigration crisis period. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

southern border. However, with respect to the estimate of the interaction of the proportion of Central American immigrants with post-immigration crisis, I am more confident. During the 2014 immigration crisis, the increase in the proportion of Central American immigrants diminished natives' positive attitude to immigration. This is consistent with the conclusion drawn in Section 4.6.1: the 2014 immigration crisis provoked a more negative attitude to immigration in the impacted areas, especially among non-Hispanic White natives.

4.8 The 2016 U.S. Presidential Election

Populism expresses negative attitudes to wealthy elites as well as anti-immigrant sentiment. Both types of attitude may be transferred to voting behavior in elections. In this section I focus on the 2016 U.S. Presidential Election, including the primaries as well as the General Election. More specifically, I examine whether the treated group relating to the Great Recession was more inclined to vote for left-wing populist Bernie Sanders, and whether the treated group relating to the immigration crisis was more likely to sup-

port right-wing populist Donald Trump. It is well known and documented that Bernie Sanders' rhetoric focused on the division between common people and corrupt wealthy elites and that he fostered a negative attitude towards those wealthy elites. At the same time, Donald Trump appealed to xenophobia by using the rhetoric of anti-(unauthorized) immigration to create a division between nationals and the immigrants who threatened them (Kazin, 2016; Rodrik, 2018b).

4.8.1 Great Recession and Left-wing Populist Voting

Since there is no information in the GSS survey about voting behavior in the U.S. Presidential Primary Elections, I utilize the information from the ANES 2016 Time Series Study and combine it with the GSS data. Since it is impossible to link the two data at the individual level, I aggregate the ANES data in the same way as I do to generate the GSS pseudo panel, and then merge it with the GSS pseudo panel at the cohort level (please refer to Section 4.3.2). The treatment group is composed of cohorts whose couple unemployment averaged within cohort in wave 2010 or 2012 is greater than or equal to 0.5.[30] In the estimation process, the controls take their values in 2010. Note that self-party identification, ideological dummy, and voting turnout are included as covariates.

The first six columns in panel a of Table 4.13 display outcomes for the 2016 U.S. Presidential Primary Elections. Regardless of controls, the treated cohorts were significantly more prone to support left-wing populist Sanders and significantly less inclined to vote for Trump, echoing the findings of Di Tella and MacCulloch (2009). These results are more likely to be due to the popularity of Sanders among the cohorts that were unemployed during the Great Recession, rather than being due merely to differences in party preferences between the treated and the controlled. Columns (7) to (10) show the estimates for the General Election in the same year as the first placebo test. After including covariates, the coefficients are insignificant and almost zero for both Clinton and Trump votes. In panel a of Table 4.14 I present results for the 2012 U.S. Presidential General Election between Obama and Romney as another placebo test. With controls, the estimates are still insignificant. Hence the support for Sanders from the treated cohorts during the Great Recession is not very likely to be explained by differences in party preference.

[30] In Table 4.A.4.5 of Appendix 4.A.4, I also directly apply the average of couple unemployment within cohort, i.e. a continuous treatment, as the explanatory variable of interest. Though the estimates are not significant, the sign and magnitude are still as expected.

Table 4.13: Effects of the Great Recession and Immigration Crisis on Populist Voting

	2016 U.S. Presid. Primary Elections						2016 U.S. Presid. General Election			
Panel a.	Sanders		Clinton		Trump		Clinton		Trump	
Great Recession	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	0.40**	0.42**	-0.10	-0.11	-0.16***	-0.25***	0.19*	0.03	-0.26***	0.01
	(0.17)	(0.18)	(0.11)	(0.14)	(0.04)	(0.09)	(0.10)	(0.18)	(0.04)	(0.13)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	135	135	135	135	135	135	136	136	136	136
Panel b.	Sanders		Clinton		Trump		Clinton		Trump	
Immigration Crisis	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	-0.15***	-0.10*	-0.08**	-0.11**	0.07**	0.09**	-0.13***	-0.12**	0.15***	0.13***
	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	132	132	132	132	132	132	134	134	134	134
Panel c.	Sanders		Clinton		Trump		Clinton		Trump	
Unemp. Pre-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	0.44**	0.19	-0.12	-0.03	-0.12*	-0.19	0.22**	0.23*	-0.21***	-0.18
	(0.17)	(0.15)	(0.11)	(0.09)	(0.07)	(0.12)	(0.09)	(0.13)	(0.07)	(0.12)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	134	134	134	134	134	134	135	135	135	135

Note: In panel a for the Great Recession, the treatment group is composed of cohorts whose couple unemployment averaged within cohort is greater than or equal to 0.5 in wave 2010 or 2012. Covariates take values in wave 2010. In panel b for the immigration crisis, the treatment group consists of cohorts that resided in the West South Central region of the U.S. in wave 2014 or 2016. Covariates take values in wave 2014. In panel c for existing unemployment pre-Great Recession, the treatment group is composed of cohorts whose couple unemployment averaged within cohort is greater than or equal to 0.5 in wave 2006 or 2008. Covariates take values in wave 2008. All the dependent variables and controls are averages within cohort. Controls contain the mean of voting turnout in the corresponding election, as well as extensive averaged demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.14: A Placebo Test: Effects of Recent Unemployment and the Immigration Crisis on the 2012 U.S. Presidential Election

	2012 U.S. Presidential General Election			
Panel a.	Obama		Romney	
Great Recession	(1)	(2)	(3)	(4)
Treatment	0.28***	0.19	-0.31***	-0.19
	(0.07)	(0.15)	(0.07)	(0.17)
Controls	No	Yes	No	Yes
No. of Obs.	134	134	134	134
Panel b.	Obama		Romney	
Immigration Crisis	(1)	(2)	(3)	(4)
Treatment	-0.09	-0.03	0.07	-0.01
	(0.06)	(0.07)	(0.06)	(0.06)
Controls	No	Yes	No	Yes
No. of Obs.	134	134	134	134

Note: In panel a for the Great Recession, the treatment group is composed of cohorts whose couple unemployment averaged within cohort is greater than or equal to 0.5 in wave 2010 or 2012. Covariates take values in wave 2010. In panel b for the immigration crisis, the treatment group consists of cohorts that resided in the West South Central region of the U.S. in wave 2014 or 2016. Covariates take values in wave 2014. All the dependent variables and controls are averages within cohort. Controls contain the mean of voting turnout in the corresponding election, as well as extensive averaged demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel c of Table 4.13 displays the estimates for the counterpart whose couple unemployment averaged within cohort in wave 2006 or 2008 is greater than or equal to 0.5. The controls take their values in 2008. Including covariates, these treated cohorts were not significantly more likely to support either left-wing populist Bernie Sanders or right-wing populist Donald Trump. They were significantly more prone to vote for Hillary Clinton who is usually regarded as a left centrist. This result is consistent with the story in Section 4.5: only recent unemployment during the Great Recession produced left-wing populism while existing unemployment from before the Great Recession did not.

4.8.2 Immigration Crisis and Right-wing Populist Voting

Likewise, the data used in this subsection also consist of the GSS pseudo panel and the ANES 2016 Time Series Study. Now the treatment group is formed by the cohorts that were in the West South Central region in wave 2014 or 2016 during the immigration crisis. Covariates take their values in wave 2014 for estimation.

Panel b of Table 4.13 reports the relevant results. During the Presidential Primary Elections, the cohorts in the West South Central region were significantly more likely

to vote for right-wing populist Trump and significantly less prone to support the two Democratic candidates Sanders and Clinton. A similar situation appeared during the Presidential General Election. These results are closely related to the conclusions of Dinas et al. (2016) and Tabellini (2018). One may suspect that they are merely a divergence between party preferences of the treatment and control groups. However, I conduct a placebo test again in panel b of Table 4.14 for the 2012 U.S. Presidential General Election, and no longer find such a divergence between the votes for Democratic candidate Obama and the votes for Republican candidate Romney. Hence support for Trump among people in the West South Central region cannot be explained, in the main, by differences in party preference.

4.8.3 Potential Mechanism

I try to distinguish two potential mechanisms through which new economic insecurity and cultural backlash led to the populist voting — accountability theory (Ferejohn, 1986) and issue ownership (Petrocik, 1996). In the current context, the former mechanism states that voters who are dissatisfied with the way the incumbent government is dealing with the Great Recession and the immigration crisis will punish it by turning to an opposition. The latter mechanism claims that dissatisfied voters will support the party or politician they deem competent on the specific issues (Dinas et al., 2016).

As shown in panel a of Table 4.13, people losing their job after the Great Recession did not decrease their support for the incumbent — the Democratic Party — or turn to the opposition — the Republican Party — in either the primaries or the General Election. Instead, these people were significantly more prone to vote for Bernie Sanders who used anti-elitist rhetoric during his campaign. Thus, accountability theory is not valid in this case, while issue ownership seems to be what appropriately explains the electoral reaction against elites and establishment after the Great Recession.

The results are mixed in panel b. The Democrats, Bernie Sanders and Hillary Clinton, both lost votes from people who were most impacted by the immigration crisis. This is predicted by accountability theory. However, Donald Trump, who used considerable anti-immigrant rhetoric and promised harsh reform of the U.S. immigration policy during his campaign, obtained increasing support from residents of the West South Central region. This is also predicted by issue ownership. In order to disentangle this puzzle, in Table 4.15 I estimate the same model for another two Republican candidates, Ted Cruz and John Kasich, during the 2016 Primary Elections. Ted Cruz held a similar anti-

immigrant position to that of Donald Trump. He was opposed to providing DREAMers (unauthorized immigrants brought to the U.S. as children) with a path to citizenship (Kapur, 2018). Moreover, he also called for the repeal of that clause of the 14th amendment granting citizenship to those born in the U.S. (Farley, 2016). However, from 2014 John Kasich changed his previous conservative opinion on immigration and called for a path to legal status for unauthorized immigrants (Sussman, 2015). In October 2015, he actually criticized Trump's plan for "building a wall along the U.S.–Mexico border and removing immigrants who entered the U.S. illegally" as "just crazy" (Rappeport, 2015). If accountability theory were true, the residents in the West South Central region would increase (or at least not decrease) votes for any Republican candidates. If issue ownership were true, these residents would more likely support candidates with a clear anti-immigration opinion. It is clear from Table 4.15 that they were significantly more prone to vote for Cruz, an anti-immigration candidate, and significantly less likely to support Kasich with his softer attitude to immigration. Once more, this phenomenon fits issue ownership rather than accountability theory.

Table 4.15: Extra Candidates in Primaries: Effects of the Immigration Crisis on Populist Voting

	2016 U.S. Presidential Primary Elections			
	Cruz		Kasich	
Immigration Crisis	(1)	(2)	(3)	(4)
Treatment	0.11***	0.08**	-0.04***	-0.05***
	(0.03)	(0.04)	(0.01)	(0.02)
Controls	No	Yes	No	Yes
No. of Obs.	132	132	132	132

Note: The treatment group consists of cohorts that resided in the West South Central region of the U.S. in wave 2014 or 2016. Covariates take values in wave 2014. All the dependent variables and controls are averages within cohort. Controls contain the mean of voting turnout in the corresponding election, as well as extensive averaged demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

In this section, I connect new economic insecurity to left-wing populist voting and link cultural anxiety to right-wing populist voting, both delivered through the channel of issue ownership. I find consistent conclusions with the literature (Rodrik, 2018b): economic deprivation drives people to support left-wing populists, and cultural concern urges them to vote for right-wing populists (Sekeris and Vasilakis, 2016; Lucassen and Lubbers, 2012). The effect of recent unemployment during the Great Recession seemed to persist in the long-term. Even five years or so after they had been made unemployed

by the Great Recession, people were still significantly more prone to vote for a left-wing populist. Such lasting negative effects on electoral support and trust are also recorded by Ananyev and Guriev (2016) and Dustmann et al. (2017).

4.9 Conclusions

Brexit, the rise of numerous radical left and radical right parties in Europe, as well as Donald Trump's presidency and Bernie Sanders's popularity during the 2016 U.S. Presidential Election, present a surge in populism. This study has investigated whether it is economic insecurity or cultural anxiety that has been driving the growth in populism.

The most important and unique contribution of this study is to identify the cause of this growth by using the Great Recession and the 2014 immigration crisis as two separate quasi-natural experiments for economic insecurity and cultural backlash respectively. This paper disentangles economic forces from cultural forces and distinguishes between left-wing populism and right-wing populism empirically. Moreover, individual longitudinal data are rarely available in attitudinal and political surveys, but they are helpful in identifying the cause here by removing individual specific confounders. With two individual level panels, my study is one of the first in populism literature that accounts for time-invariant unobserved heterogeneity and time-varying observable characteristics. Compared with earlier studies, a larger set of measures is employed to capture the economic and political dimensions of populism.

I find that recent unemployment during the Great Recession, rather than existing unemployment from before the recession, is what engendered attitudes against wealthy elites, such as a decrease in trust in those people who manage big companies and a rise in preferences for income redistribution by imposing higher taxes on wealthy families rather than by providing financial assistance to poor people. These anti-elitist attitudes are connected with left-wing populism in the literature. This result is original in that it distinguishes between recent unemployment during the Great Recession and existing unemployment from before the recession. Individuals who became unemployed during the Great Recession perceived the economy as manipulated by elites and thus unjust to them. However, those who had lost their job before the Great Recession did not express that perception.

The current study also provides quasi-natural experimental evidence that unauthorized immigration generated a more negative attitude to immigration in the more intensely affected region. Such anti-immigration attitude was driven by cultural and iden-

tity concerns rather than labor market competition and social security crowding out. This phenomenon is related to right-wing populism in the literature.

Furthermore, new unemployment amid the Great Recession and the 2014 immigration crisis influenced the 2016 U.S. Presidential Election. Cohorts with a high average of unemployment post-Great Recession preferred left-wing populist Bernie Sanders, while cohorts in the most intensely impacted region during the immigration crisis supported right-wing populist Donald Trump. Placebo tests verify that these voting consequences could not be merely attributed to difference in party preferences.

In sum, this paper takes a step towards reconciling the economic insecurity perspective with the cultural backlash thesis. Economic insecurity represented by recent unemployment during the Great Recession drove left-wing populism while cultural anxiety due to unauthorized immigration triggered right-wing populism. These implications are helpful for economists and policy makers who seek to prevent or direct the influence of populism in economic and political institutions.

Appendix 4.A

Appendix 4.A.1: Definitions and Descriptives of Variables

Table 4.A.1.1 provides an overview of the definitions of variables used in the analysis. Table 4.A.1.2 presents the descriptives of these variables in the 2006 sample panel (wave 2006 to 2010) for the Great Recession. Table 4.A.1.3 shows the descriptives in the 2010 sample panel (wave 2010 to 2014) for the immigration crisis.

Table 4.A.1.1: Definitions of Variables

Variable	Definition
Trust companies	Confidence in people running major companies (one to three)
Gov. equal. inc.	U.S. gov. ought to reduce income difference between rich and poor (one to seven)
Gov. help poor	U.S. gov. should improve living standard of poor (one to five)
Attitude to immig.	Dummy variable if number of immigrants to U.S. should not decrease
Unemploy. couple	Dummy variable if either partner of couple unemployed
Econ. unfairness	Luck & network more important than hard work in one's success (one to three)
Married	Dummy variable if married
Widowed	Dummy variable if widowed
Divorced	Dummy variable if divorced
Separated	Dummy variable if legally separated, i.e. legally living apart but still legally married
Never married	Dummy variable if never married
Children number	Number of children
Sibling number	Number of siblings
Age	Age of respondent
Age squared	Age squared of respondent
Lower high school	Dummy variable if highest degree is lower than high school
High school	Dummy variable if highest degree is high school
College	Dummy variable if highest degree is junior college or bachelor
Graduate	Dummy variable if highest degree is graduate degree
Family income	Categories of total family income before taxes last year
City size	Categories of population size of respondent's place
Democrat	Dummy variable if party self-identification is democrat
Republican	Dummy variable if party self-identification is republican
Liberal	Dummy variable if political view is liberal
Home owner	Dummy variable if home owner

Table 4.A.1.2: Descriptives in the 2006 Sample Panel; Wave 2006 – 2010

	Treatment			Control		
Variables	Mean	Minimum	Maximum	Mean	Minimum	Maximum
Trust companies	1.98	1	3	1.94	1	3
Gov. equal. inc.	4.45	1	7	4.22	1	7
Gov. help poor	3.29	1	5	3.06	1	5
Econ. unfairness	1.51	1	3	1.47	1	3
Number of children	2.07	0	6	1.95	0	8
Number of siblings	3.96	0	25	3.51	0	32
Age	43.31	22	75	50.06	18	89
Percentages						
Attitude to immig.	0.50	0	1	0.47	0	1
Married	0.54	0	1	0.51	0	1
Widowed	0.02	0	1	0.09	0	1
Divorced	0.16	0	1	0.17	0	1
Separated	0.08	0	1	0.03	0	1
Never married	0.20	0	1	0.20	0	1
Lower high school	0.11	0	1	0.11	0	1
High school	0.57	0	1	0.49	0	1
College	0.26	0	1	0.28	0	1
Graduate	0.06	0	1	0.12	0	1
Family income 10K minus	0.16	0	1	0.17	0	1
Family income 10-20K	0.13	0	1	0.09	0	1
Family income 20-30K	0.13	0	1	0.10	0	1
Family income 30-40K	0.11	0	1	0.11	0	1
Family income 40-50K	0.09	0	1	0.08	0	1
Family income 50-60K	0.07	0	1	0.09	0	1
Family income 60-75K	0.08	0	1	0.10	0	1
Family income 75-90K	0.07	0	1	0.07	0	1
Family income 90-110K	0.05	0	1	0.06	0	1
Family income 110-150K	0.07	0	1	0.07	0	1
Family income 150K plus	0.04	0	1	0.06	0	1
City size 10K minus	0.39	0	1	0.32	0	1
City size 10-100K	0.37	0	1	0.45	0	1
City size 100-1000K	0.16	0	1	0.16	0	1
City size 1000K plus	0.08	0	1	0.07	0	1
Democrat	0.38	0	1	0.34	0	1
Republican	0.19	0	1	0.27	0	1
Liberal	0.26	0	1	0.27	0	1
Home owner	0.38	0	1	0.47	0	1

Based on 81 individuals in the treatment group and 1,109 in the control group.

Table 4.A.1.3: Descriptives in the 2010 Sample Panel; Wave 2010 – 2014

	Treatment			Control		
Variable	Mean	Minimum	Maximum	Mean	Minimum	Maximum
Number of children	2.19	0	8	1.84	0	8
Number of siblings	4.42	0	20	3.36	0	30
Age	46.78	19	89	49.77	18	89
Percentages						
Attitude to immig.	0.52	0	1	0.49	0	1
Unemploy. couple	0.08	0	1	0.07	0	1
Married	0.43	0	1	0.46	0	1
Widowed	0.08	0	1	0.08	0	1
Divorced	0.16	0	1	0.19	0	1
Separated	0.05	0	1	0.03	0	1
High school	0.54	0	1	0.50	0	1
College	0.23	0	1	0.28	0	1
Graduate	0.04	0	1	0.13	0	1
Family income 10-20K	0.19	0	1	0.10	0	1
Family income 20-30K	0.18	0	1	0.10	0	1
Family income 30-40K	0.10	0	1	0.09	0	1
Family income 40-50K	0.07	0	1	0.08	0	1
Family income 50-60K	0.07	0	1	0.08	0	1
Family income 60-75K	0.08	0	1	0.09	0	1
Family income 75-90K	0.03	0	1	0.07	0	1
Family income 90-110K	0.03	0	1	0.07	0	1
Family income 110-150K	0.03	0	1	0.08	0	1
Family income 150K plus	0.06	0	1	0.08	0	1
City size 10-100K	0.24	0	1	0.46	0	1
City size 100-1000K	0.34	0	1	0.17	0	1
City size 1000K plus	0.10	0	1	0.07	0	1
Democrat	0.45	0	1	0.35	0	1
Republican	0.16	0	1	0.24	0	1
Liberal	0.25	0	1	0.29	0	1
Home owner	0.51	0	1	0.53	0	1

Based on 136 individuals in the treatment group and 1,168 in the control group.

Appendix 4.A.2: Covariate Balance Pre-Shock

Table 4.A.2.1 compares covariates between the treatment group and the control group, measured pre-Great Recession, in the 2006 sample panel. Table 4.A.2.2 compares covariates between the treated and controlled, measured pre-immigration crisis, in the 2010 sample panel.

Table 4.A.2.1: Descriptives in the 2006 Sample Panel; Pre-Great Recession Wave 2006 – 2008

	Treatment			Control			Diff. test p-value
Variables	Mean	Min.	Max.	Mean	Min.	Max.	based on matched sample
Number of children	2.00	0	6	1.92	0	8	0.30
Number of siblings	3.85	0	14	3.50	0	32	0.93
Age	42.24	22	73	49.04	18	89	0.22
Percentages							
Married	0.56	0	1	0.52	0	1	0.11
Widowed	0.01	0	1	0.08	0	1	0.71
Divorced	0.17	0	1	0.17	0	1	0.42
Separated	0.07	0	1	0.02	0	1	0.88
Never married	0.19	0	1	0.21	0	1	0.18
High school	0.57	0	1	0.49	0	1	0.89
College	0.27	0	1	0.28	0	1	0.11
Graduate	0.05	0	1	0.12	0	1	0.79
Family income 10K minus	0.18	0	1	0.16	0	1	0.49
Family income 10-20K	0.15	0	1	0.09	0	1	0.21
Family income 20-30K	0.09	0	1	0.10	0	1	0.60
Family income 30-40K	0.09	0	1	0.11	0	1	0.96
Family income 40-50K	0.10	0	1	0.08	0	1	0.93
Family income 50-60K	0.08	0	1	0.10	0	1	0.69
Family income 60-75K	0.07	0	1	0.10	0	1	0.53
Family income 75-90K	0.08	0	1	0.07	0	1	0.62
Family income 90-110K	0.06	0	1	0.06	0	1	0.31
Family income 110-150K	0.07	0	1	0.07	0	1	0.79
Family income 150K plus	0.03	0	1	0.06	0	1	0.81
City size 10K minus	0.40	0	1	0.32	0	1	0.41
City size 10-100K	0.36	0	1	0.45	0	1	0.47
City size 100-1000K	0.17	0	1	0.16	0	1	0.25
City size 1000K plus	0.07	0	1	0.07	0	1	0.12
Democrat	0.37	0	1	0.35	0	1	0.20
Republican	0.20	0	1	0.27	0	1	0.38
Liberal	0.27	0	1	0.27	0	1	0.27
Home owner	0.37	0	1	0.47	0	1	0.46

Based on 81 individuals in the treatment group and 1,109 in the control group.

Table 4.A.2.2: Descriptives in the 2010 Sample Panel; Pre-Immigration Crisis Wave 2010 – 2012

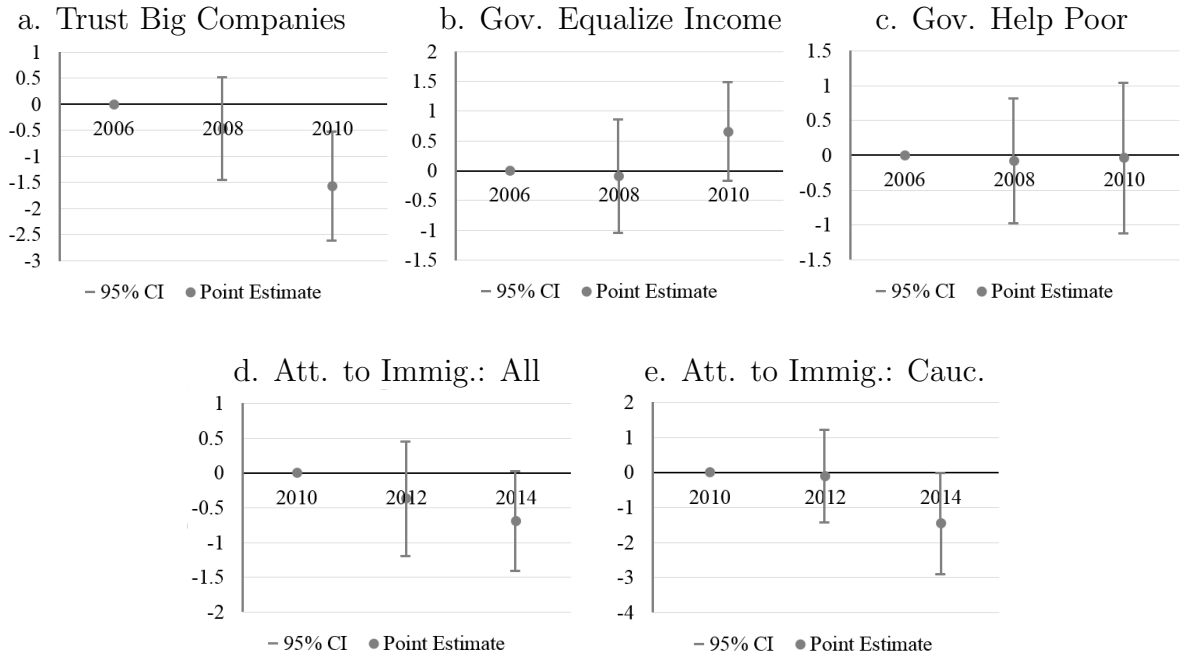
	Treatment			Control			Diff. test p-value
Variables	Mean	Min.	Max.	Mean	Min.	Max.	based on matched sample
Number of children	2.17	0	8	1.81	0	8	0.99
Number of siblings	4.43	0	19	3.36	0	30	0.22
Age	45.80	19	88	48.77	18	89	0.77
Percentages							
Married	0.43	0	1	0.46	0	1	0.89
Widowed	0.07	0	1	0.08	0	1	0.88
Divorced	0.16	0	1	0.19	0	1	0.97
Separated	0.04	0	1	0.03	0	1	0.68
High school	0.56	0	1	0.50	0	1	0.59
College	0.23	0	1	0.28	0	1	0.52
Graduate	0.04	0	1	0.13	0	1	0.30
Family income 10-20K	0.21	0	1	0.11	0	1	0.05
Family income 20-30K	0.18	0	1	0.09	0	1	0.38
Family income 30-40K	0.09	0	1	0.10	0	1	0.51
Family income 40-50K	0.08	0	1	0.08	0	1	0.52
Family income 50-60K	0.06	0	1	0.08	0	1	0.60
Family income 60-75K	0.08	0	1	0.09	0	1	0.26
Family income 75-90K	0.03	0	1	0.07	0	1	0.20
Family income 90-110K	0.02	0	1	0.07	0	1	0.38
Family income 110-150K	0.04	0	1	0.07	0	1	0.52
Family income 150K plus	0.05	0	1	0.08	0	1	0.48
City size 10-100K	0.23	0	1	0.46	0	1	0.17
City size 100-1000K	0.34	0	1	0.17	0	1	0.11
City size 1000K plus	0.10	0	1	0.07	0	1	0.83
Democrat	0.44	0	1	0.35	0	1	0.03
Republican	0.17	0	1	0.24	0	1	0.05
Liberal	0.23	0	1	0.29	0	1	0.47
Home owner	0.42	0	1	0.45	0	1	0.76

Based on 136 individuals in the treatment group and 1,168 in the control group.

Appendix 4.A.3: Pre-Treatment Trends

Figure 4.A.3.1 illustrates the coefficient estimates of the leads and lags model for different outcome variables. None of the estimates of the interactions of $Treatment$ and the second wave is significantly distinguishable from zero. Therefore, arguably, the trends measured pre-shock between the treatment and control groups are parallel.

Figure 4.A.3.1: Pre-Treatment Trends: Estimates of Treatment×Every Wave



Appendix 4.A.4: Supplementary Estimation Tables

Table 4.A.4.1: Effects of Recent Unemployment during the Great Recession on Attitudes Related to Populism; Full Baseline Model

	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Recent Unemp. Post-Rec.	(1)		(2)		(3)		(4)	
Treat.×Post-Great Rec.	-1.32***	(0.50)	0.70**	(0.34)	-0.00	(0.43)	-0.28	(0.48)
Married	-0.47	(0.50)	-0.03	(0.39)	-0.08	(0.35)	-0.99	(0.69)
Widowed	-1.24	(0.80)	0.64	(0.62)	-0.16	(0.56)	-0.11	(0.79)
Divorced	-0.88	(0.61)	0.11	(0.54)	-0.05	(0.51)	-0.38	(0.80)
Separated	-0.81	(0.67)	-0.58	(0.54)	0.28	(0.52)	-1.02	(0.81)
Number of Siblings	0.13**	(0.06)	-0.00	(0.06)	0.12**	(0.06)	0.08	0.08
Number of Children	-0.03	(0.14)	-0.00	(0.13)	0.09	(0.12)	-0.01	(0.18)
Age	-0.12	(0.09)	0.02	(0.09)	0.01	(0.10)	-0.48**	(0.21)
Age Squared	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00*	(0.00)
High School	0.53	(0.50)	-0.12	(0.39)	0.64	(0.52)	-0.85	(0.72)
College	1.12*	(0.60)	-0.06	(0.46)	0.42	(0.59)	-0.54	(0.82)
Graduate	1.14	(0.69)	0.38	(0.63)	0.58	(0.72)	-0.74	(1.04)
Family Income 10-20K	0.29	(0.30)	0.21	(0.28)	-0.51*	(0.28)	-0.57	(0.35)
Family Income 20-30K	0.06	(0.30)	0.24	(0.25)	-0.07	(0.28)	0.48	(0.37)
Family Income 30-40K	-0.16	(0.29)	0.30	(0.24)	-0.06	(0.28)	0.37	0.37
Family Income 40-50K	0.24	(0.31)	0.27	(0.29)	-0.15	(0.29)	-0.29	(0.40)
Family Income 50-60K	0.15	(0.31)	-0.12	(0.27)	-0.46	(0.28)	0.14	(0.37)
Family Income 60-75K	0.09	(0.31)	-0.33	(0.27)	-0.36	(0.28)	0.59	(0.42)
Family Income 75-90K	-0.05	(0.35)	-0.10	(0.32)	-0.36	(0.32)	0.21	(0.45)
Family Income 90-110K	0.14	(0.34)	-0.20	(0.33)	0.07	(0.37)	-0.21	(0.53)
Family Income 110-150K	-0.34	(0.38)	-0.01	(0.32)	0.01	(0.37)	0.17	(0.54)
Family Income 150K Plus	0.35	(0.39)	-0.56	(0.37)	-0.18	(0.39)	-0.19	(0.56)
City Size 10-100K	-0.15	(0.31)	0.39	(0.28)	0.57*	(0.33)	0.22	(0.35)
City Size 100-1000K	-0.05	(0.56)	0.38	(0.55)	0.62	(0.51)	0.33	(0.59)
City Size 1000K Plus	-1.75*	(0.97)	1.57	(1.05)	2.01*	(1.07)	-0.36	(1.39)
Democrat	0.11	(0.25)	0.14	(0.22)	0.15	(0.24)	-0.37	(0.33)
Republican	0.16	(0.26)	0.16	(0.24)	0.05	(0.21)	0.15	(0.33)
Liberal	-0.16	(0.18)	0.07	(0.16)	0.05	(0.19)	0.36	(0.23)
Home Owner	-0.24	(0.40)	-0.01	(0.32)	-0.19	(0.28)	0.15	(0.31)
Wave 2	0.03	(0.15)	-0.04	(0.13)	-0.03	(0.16)	0.74**	(0.33)
Wave 3	-0.46**	(0.20)	-0.78***	(0.20)	-0.37	(0.25)	1.47***	(0.56)
No. of Obs.	1,569		5,049		3,057		878	

Note: The sample excludes individuals who were unemployed or whose spouse was unemployed in either wave 2006 or wave 2008. The treatment group consists of respondents who did not become unemployed or whose spouse did not become unemployed until wave 2010 and the control group contains respondents who were not laid off, and whose spouse was not laid off, at all in the three waves. Wave 2010 is the only post-Great Recession period with respect to unemployment. Individual fixed effects and survey wave fixed effects are included in every column. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.A.4.2: Effects of the Immigration Crisis on Attitude to Immigration; Full Baseline Model

Attitude to Immigration	All		Non-Hispanic Whites	
	(1)		(2)	
Treat.×Post-Immig. Crisis	-0.50*	(0.30)	-1.40**	(0.60)
Married	-0.01	(0.42)	-0.07	(0.73)
Widowed	-0.18	(0.69)	-0.11	(1.24)
Divorced	-0.50	(0.53)	-0.82	(0.95)
Separated	-0.19	(0.56)	-0.50	(1.13)
Number of Siblings	-0.04	(0.06)	0.01	(0.11)
Number of Children	0.00	(0.15)	-0.20	(0.19)
Age	-0.01	(0.11)	0.09	(0.15)
Age Squared	-0.00	(0.00)	-0.00	(0.00)
High School	0.13	(0.39)	0.50	(0.76)
College	-0.36	(0.52)	0.03	(0.82)
Graduate	-1.01	(0.77)	-2.00	(1.26)
Family Income 10-20K	0.21	(0.29)	0.11	(0.46)
Family Income 20-30K	0.09	(0.33)	-0.31	(0.52)
Family Income 30-40K	0.11	(0.32)	0.07	(0.46)
Family Income 40-50K	-0.20	(0.35)	0.13	(0.50)
Family Income 50-60K	0.11	(0.39)	0.02	(0.53)
Family Income 60-75K	-0.01	(0.41)	-0.11	(0.53)
Family Income 75-90K	-0.16	(0.47)	-0.50	(0.59)
Family Income 90-110K	-0.11	(0.48)	-0.51	(0.60)
Family Income 110-150K	-1.15**	(0.50)	-1.38**	(0.64)
Family Income 150K Plus	-0.33	(0.47)	-0.62	(0.53)
City Size 10-100K	0.05	(0.38)	0.64	(0.52)
City Size 100-1000K	0.47	(0.75)	-0.13	(0.84)
City Size 1000K Plus	1.15	(0.93)	2.53**	(1.16)
Democrat	-0.06	(0.25)	-0.10	(0.36)
Republican	-0.26	(0.28)	-0.34	(0.37)
Liberal	0.13	(0.21)	0.16	(0.32)
Home Owner	0.16	(0.30)	0.30	(0.45)
Wave 2	0.24	(0.19)	0.35	(0.25)
Wave 3	0.76***	(0.28)	1.02***	(0.39)
No. of Obs.	1,070		690	

Note: The treatment group is composed of respondents who resided in the West South Central region of the U.S. in wave 2014, i.e. the states of Arkansas, Louisiana, Oklahoma, and Texas, that the immigration crisis most intensely impacted. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. The first two columns use the whole sample and the last two restrict the sample to include non-Hispanic Whites only. Individual fixed effects and survey wave fixed effects are included in every column. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.A.4.3: Effects of Recent Unemployment on Attitudes Related to Populism: Location-Specific Trends & Placebo Treatment

	Trust Companies		Gov. Equ. Inc.		Gov. Help Poor		Atti. Immig.	
Recent Unemp. Post-Rec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat.×Post-Great Rec.	-1.35**	-0.41	0.77**	-0.07	-0.08	0.00	-0.06	-0.81
	(0.56)	(0.49)	(0.33)	(0.43)	(0.44)	(0.51)	(0.56)	(0.58)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	1,569	682	5,049	2,148	3,057	1,298	878	398

Note: The odd columns report estimates of the specification including state-specific linear trends. The even columns show estimates with wave 2008 as a fake treatment time by using only pre-(real) treatment data, i.e. the first two waves. The treatment group consists of respondents who did not become unemployed or whose spouse did not become unemployed until wave 2010 and the control group contains respondents who were not laid off, and whose spouse was not laid off, at all in the three waves. Wave 2010 is the only post-Great Recession period with respect to unemployment. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.A.4.4: Effects of the Immigration Crisis on Attitude to Immigration: Different Coverage of Treated Region

	Extended Treated Region		Shrunk Treated Region	
	All	Non-Hisp. Whites	All	Non-Hisp. Whites
Attitude to Immigration	(1)	(2)	(3)	(4)
Treat.×Post-Immig. Crisis	-0.40	-1.11*	-0.45	-0.71
	(0.30)	(0.58)	(0.37)	(0.68)
Controls	Yes	Yes	Yes	Yes
Number of observations	1,070	690	1,070	690

Note: In the first two columns, the treatment group is extended to contain respondents who resided in wave 2014 in all states bordering with Mexico including Arizona, New Mexico, Texas and neighboring states of Texas such as Arkansas, Louisiana, and Oklahoma. In the last two columns, the treatment group is shrunk to contain respondents who resided in wave 2014 in New Mexico and Texas only. The control group covers the rest of the U.S. in the same wave. The post-immigration crisis period is wave 2014. Odd columns include the whole sample and even columns restricts the sample to include non-Hispanic Whites only. Individual fixed effects and survey wave fixed effects are included in every column. Controls contain extensive demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors clustered at the individual level are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.A.4.5: Effects of the Great Recession on Populist Voting: Cohort Mean of Couple Unemployment as Explanatory Variable

	2016 U.S. Presid. Primary Elections						2016 U.S. Presid. General Election			
	Sanders		Clinton		Trump		Clinton		Trump	
Great Recession	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cohort mean of couple unemp.	0.32**	0.25	-0.11	-0.14	-0.16**	-0.16	0.16	-0.02	-0.30***	-0.04
	(0.15)	(0.22)	(0.12)	(0.15)	(0.08)	(0.11)	(0.11)	(0.16)	(0.09)	(0.14)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
No. of Obs.	135	135	135	135	135	135	136	136	136	136

Note: Covariates take values in wave 2010. All the dependent and explanatory variables are averages within cohort. Controls contain the mean of voting turnout in the corresponding election, as well as extensive averaged demographic and socio-economic variables such as the quadratic of respondent's age, marital status dummies, number of siblings, number of children, academic degree dummies, categories of last year's total family income, categories of the population size of respondent's place, party self-identification, dummy of liberal ideology, and dummy of home owner. Robust standard errors in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Appendix 4.B: Numbers of Family Unit Apprehensions by Month

Table 4.B.1 lists the numbers of family unit apprehensions by month in different border patrol sectors in the U.S. from October 2012 to September 2016.

Table 4.B.1: Total Family Unit Apprehensions by Month; Oct.2012-Sep.2016

a. Fiscal Year 2013

SECTOR	October	November	December	January	February	March	April	May	June	July	August	September	Yearly Total
Miami	12	1	2	6	0	1	1	5	6	5	2	2	43
New Orleans	7	2	4	0	0	0	0	0	0	0	2	3	18
Ramey	2	0	0	0	1	1	0	0	0	0	1	1	6
Blaine	0	1	3	2	3	2	3	1	1	8	4	2	30
Buffalo	1	0	0	0	4	0	0	1	0	10	2	1	19
Detroit	0	0	1	0	0	0	0	0	0	2	2	1	6
Grand Forks	0	0	2	0	0	1	1	0	0	0	0	0	4
Havre	2	0	0	0	0	0	0	0	0	0	0	0	2
Houlton	0	0	0	0	0	0	0	0	0	0	0	0	0
Spokane	9	0	0	0	0	0	0	0	0	1	0	0	10
Swanton	5	3	1	0	0	5	2	13	3	13	5	13	63
Big Bend *(formerly Marfa)*	16	10	9	11	9	3	11	8	3	10	7	5	102
Del Rio	17	26	34	16	34	37	50	55	85	98	139	120	711
El Centro	36	33	23	34	19	38	54	43	29	18	20	18	365
El Paso	29	26	30	18	24	30	26	44	17	15	31	8	298
Laredo	105	114	101	110	98	178	139	155	110	211	193	174	1,688
Rio Grande Valley *(formerly McAllen)*	266	278	231	236	310	484	606	637	698	1,016	1,240	1,263	7,265
San Diego	88	86	110	153	155	177	160	131	119	126	119	152	1,576
Tucson	211	178	183	245	265	336	310	224	178	151	155	194	2,630
Yuma	31	25	25	24	9	27	28	18	11	6	3	13	220
Coastal Border	21	3	6	6	1	2	1	5	6	5	5	6	67
Northern Border	17	4	7	2	7	8	6	15	4	34	13	17	134
Southwest Border	799	776	746	847	923	1,310	1,384	1,315	1,250	1,651	1,907	1,947	14,855
Monthly Total	837	783	759	855	931	1,320	1,391	1,335	1,260	1,690	1,925	1,970	15,056

b. Fiscal Year 2014

SECTOR	October	November	December	January	February	March	April	May	June	July	August	September	Yearly Total
Miami	4	2	4	17	16	5	1	0	12	5	8	13	87
New Orleans	0	1	0	6	4	1	9	2	9	2	1	1	36
Ramey	0	1	0	0	0	0	0	0	0	0	1	0	2
Blaine	0	0	0	3	1	0	3	6	0	2	1	0	16
Buffalo	0	0	0	1	0	2	2	0	0	2	4	1	12
Detroit	0	1	0	0	0	1	2	2	2	0	0	0	8
Grand Forks	1	3	0	1	2	1	1	0	0	0	3	2	14
Havre	0	0	0	0	0	0	0	0	0	0	0	0	0
Houlton	0	0	0	0	0	0	0	0	0	0	2	0	2
Spokane	0	0	0	0	0	0	1	2	2	0	0	0	5
Swanton	1	0	12	2	0	1	6	4	2	20	2	7	57
Big Bend *(formerly Marfa)*	4	1	8	12	11	3	7	8	28	58	25	11	176
Del Rio	150	172	185	179	311	521	467	1,080	1,134	466	173	112	4,950
El Centro	37	21	51	50	36	25	48	59	105	119	41	38	630
El Paso	23	30	29	26	49	44	45	60	113	72	39	32	562
Laredo	164	176	255	171	262	351	315	554	739	316	183	105	3,591
Rio Grande Valley *(formerly McAllen)*	1,472	1,953	2,264	1,509	2,246	4,306	5,098	10,145	13,370	5,792	2,467	1,704	52,326
San Diego	171	129	130	149	106	187	146	175	168	119	137	106	1,723
Tucson	375	294	373	166	185	235	320	576	592	376	176	144	3,812
Yuma	18	10	16	24	75	80	65	115	81	87	55	49	675
Coastal Border	4	4	4	23	20	6	10	2	21	7	10	14	125
Northern Border	2	4	12	7	3	5	15	14	6	24	12	10	114
Southwest Border	2,414	2,786	3,311	2,286	3,281	5,752	6,511	12,772	16,330	7,405	3,296	2,301	68,445
Monthly Total	2,420	2,794	3,327	2,316	3,304	5,763	6,536	12,788	16,357	7,436	3,318	2,325	68,684

c. Fiscal Year 2015

SECTOR	October	November	December	January	February	March	April	May	June	July	August	September	Yearly Total
Miami	6	4	17	1	0	1	6	4	10	5	30	14	98
New Orleans	3	3	8	1	3	0	0	1	2	1	3	0	25
Ramey	0	0	0	0	0	0	2	0	1	4	0	1	8
Blaine	4	10	1	6	2	0	0	5	6	0	3	0	37
Buffalo	0	0	0	1	0	1	0	0	1	0	0	0	3
Detroit	0	0	3	0	0	0	0	0	0	0	1	0	4
Grand Forks	1	0	0	0	2	0	2	1	2	1	0	0	9
Havre	0	0	0	0	0	0	0	0	0	0	0	0	0
Houlton	0	0	0	0	0	0	0	0	0	1	0	0	1
Spokane	0	0	0	0	0	0	0	0	0	0	0	5	5
Swanton	3	2	8	1	1	2	0	0	0	5	2	1	25
Big Bend *(formerly Marfa)*	30	15	31	14	25	21	40	60	49	103	192	227	807
Del Rio	79	83	118	95	72	182	174	269	227	233	322	287	2,141
El Centro	16	47	83	22	31	53	76	38	59	76	79	95	675
El Paso	22	27	45	22	19	67	149	118	144	213	185	209	1,220
Laredo	136	112	108	104	76	90	87	97	113	126	138	185	1,372
Rio Grande Valley *(formerly McAllen)*	1,556	1,809	1,979	1,091	1,404	1,834	2,018	2,584	2,904	3,106	3,577	3,547	27,409
San Diego	119	123	185	129	126	176	133	159	102	85	111	102	1,550
Tucson	180	164	276	95	225	256	296	333	254	258	265	328	2,930
Yuma	24	35	66	50	63	103	114	203	190	303	290	293	1,734
Coastal Border	9	7	25	2	3	1	8	5	13	10	33	15	131
Northern Border	8	12	12	8	5	3	2	6	9	7	6	6	84
Southwest Border	2,162	2,415	2,891	1,622	2,041	2,782	3,087	3,861	4,042	4,503	5,159	5,273	39,838
Monthly Total	2,179	2,434	2,928	1,632	2,049	2,786	3,097	3,872	4,064	4,520	5,198	5,294	40,053

d. Fiscal Year 2016

SECTOR	October	November	December	January	February	March	April	May	June	July	August	September	Yearly Total
Miami	8	2	12	5	3	8	14	0	3	10	3	10	78
New Orleans	6	0	0	0	6	2	1	0	2	0	3	0	20
Ramey	0	2	3	0	0	2	3	0	3	4	1	0	18
Blaine	0	0	0	0	0	3	3	2	4	8	5	4	29
Buffalo	3	0	0	0	0	0	2	0	0	0	0	0	5
Detroit	2	0	0	0	0	0	0	0	0	1	0	0	3
Grand Forks	0	0	0	0	0	3	0	0	0	0	0	0	3
Havre	0	0	0	0	0	0	0	0	0	0	0	0	0
Houlton	0	0	0	0	0	0	0	0	0	0	0	0	0
Spokane	0	0	0	0	0	0	0	2	0	0	0	0	2
Swanton	3	0	3	2	4	0	0	0	4	0	2	7	25
Big Bend *(formerly Marfa)*	240	123	166	53	41	44	29	76	43	47	97	92	1,051
Del Rio	283	314	539	174	188	193	240	397	226	353	293	349	3,549
El Centro	89	110	164	42	47	76	120	155	104	180	211	295	1,593
El Paso	266	424	751	104	152	226	349	433	473	616	866	1,004	5,664
Laredo	151	160	190	130	102	155	151	119	103	119	135	125	1,640
Rio Grande Valley *(formerly McAllen)*	4,172	4,356	5,809	2,020	1,890	3,051	3,851	4,568	4,568	5,038	6,341	6,342	52,006
San Diego	108	134	233	203	194	187	245	200	243	309	372	435	2,863
Tucson	303	376	453	166	104	216	174	257	234	280	333	243	3,139
Yuma	413	474	668	251	332	303	461	578	633	627	705	724	6,169
Coastal Border	14	4	15	5	9	12	18	0	8	14	7	10	116
Northern Border	8	0	3	2	4	6	5	4	8	9	7	11	67
Southwest Border	6,025	6,471	8,973	3,143	3,050	4,451	5,620	6,783	6,627	7,569	9,353	9,609	77,674
Monthly Total	6,047	6,475	8,991	3,150	3,063	4,469	5,643	6,787	6,643	7,592	9,367	9,630	77,857

Source: United States Border Patrol

Appendix 4.C: Details of Survey Questions on Variables

The questions on the confidence in major companies read "I am going to name major companies in this country. As far as the people running these major companies are concerned, would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in them?" and the responses contain "1. A great deal, 2. Only some, 3. Hardly any, 8. Don't know, 9. No answer, IAP. Not applicable". Two questions concern the government's income redistribution policies. The first one asks "Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor. Here is a card with a scale from 1 to 7. Think of a score of 1 as meaning that the government ought to reduce the income differences between rich and poor, and a score of 7 meaning that the government should not concern itself with reducing income differences. What score between 1 and 7 comes closest to the way you feel? (CIRCLE ONE):" and the answers range from 1 to 7 as well as "8. Don't know, 9. No answer, IAP. Not applicable". The second relevant question is "Some people think that the government in Washington should do everything possible to improve the standard of living of all poor Americans; they are at Point 1 on this card. Other people think it is not the government's responsibility, and that each person should take care of himself; they are at Point 5. Where would you place yourself on this scale, or haven't you have up your mind on this?" and the responses range from 1 to 5 as well as "8. Don't know, 9. No answer, IAP. Not applicable". The question regarding the perception of economic unfairness is "Some people say that people get ahead by their own hard work; others say that lucky breaks or help from other people are more important. Which do you think is most important?" and the answers are "1. Hard work most important, 2. Hard work, luck equally important, 3. Luck most important, 8. Don't know, 9. No answer, IAP. Not applicable". The question on attitude to immigration reads "Do you think the number of immigrants to America nowadays should be" and the corresponding answers provided are "1. increased a lot, 2. increased a little, 3. remain the same as it is, 4. reduced a little, 5. reduced a lot, 8. can't choose, 9. no answer, and IAP. not applicable". I see all the answers of "8. Don't know, 9. No answer, IAP. Not applicable" as missing values.

All these outcome variables are inverted so that a larger score in each outcome vari-

able refers to a higher level of confidence in major corporations, preferences for income redistribution, and the perception of economic unfairness, respectively. The dummy variable of the attitude to immigration is coded in the way such that it values 1 if respondent reported "1. increased a lot, 2. increased a little, 3. remain the same as it is", and values 0 otherwise.

The questions concerning the working status ask "Last week were you (your wife/husband) working full time, part time, going to school, keeping house, or what?" respectively. The answers provided are "1. Working full time, 2. Working part time, 3. With a job, but not at work because of temporary illness, vacation, strike, 4. Unemployed, laid off, looking for work, 5. Retired, 6. In school, 7. Keeping house, 8. Other, 9. No answer, IAP. Not applicable (for spousal working status only)". The couple unemployment variable is coded in the way such that it values 1 if the respondent reported "4. Unemployed, laid off, looking for work" for him/herself or for his or her spouse, and values 0 otherwise.

Appendix 4.D: Game Theoretical Framework

In this appendix I present a simple game theoretical model to explain the key findings in the main body of the paper. This model is a modified version based on the one developed by Ghatak and Verdier (2017).

4.D.1: Set-Up of Homogeneous Society

This set-up of homogeneous-player game is established by Ghatak and Verdier (2017). Consider a one-stage game in which both players, a and b, have to divide a positive payoff R. They both can take an action from the action set $A = \{F, NF\}$ where F refers to fight and NF indicates non-fight. In a homogenous society, every member is assumed to be identical thus with the same probability of winning, namely $\frac{1}{2}$, if they choose to fight. The payoff in the situation of mutual fighting will be divided evenly between the two players with a cost of fighting, c such that $c > 0$, borne by both players. One may see this cost as an opportunity cost such as time or energy that would have been spent on profitable events, e.g., work. If one player fights while the other surrenders, then the whole payoff will be taken by the fighter. If neither players decides to fight, then the payoff will be evenly shared by the two players with an extra reward, r such that $r > 0$, enjoyed by both players. This extra reward may be regarded as beneficial networks or phycological benefits such as mutual trust and subjective well-being. The payoffs in different situations are shown in Table 4.D.1.

Table 4.D.1: Game of Homogeneous Society

a \ b	F	NF
F	$(\frac{1}{2}R - c, \frac{1}{2}R - c)$	$(R, 0)$
NF	$(0, R)$	$(\frac{1}{2}R + r, \frac{1}{2}R + r)$

With different parameter values, this game may transform to divergent variants: if $\frac{R}{2} - c > 0$ and $r < \frac{R}{2}$, it will become the Prisoner's Dilemma. The only equilibrium will be that both players decide to fight and obtain their payoff $\frac{R}{2} - c$, respectively; if $\frac{R}{2} - c < 0$ and $r < \frac{R}{2}$, it will be the Game of Chicken or Hawk-Dove Game. The two pure equilibria are that one player fights and obtains the whole payoff R while the other concedes and gets nothing; if $\frac{R}{2} - c > 0$ and $r > \frac{R}{2}$, it will become a coordination game. The two pure equilibria are that both players choose the same action, either fight or compromise, while

in the former case both acquire $\frac{R}{2} - c$ and in the latter both get $\frac{R}{2} + r$; if $\frac{R}{2} - c < 0$ and $r > \frac{R}{2}$, the only equilibrium will be that both players compromise and acquire $\frac{R}{2} + r$, respectively.

4.D.2: Society of Heterogeneous Socio-Economic Classes

Now suppose that members of the society are divided to two groups: group A with low socio-economic class, and group B with high socio-economic class. A is assumed to be more advantageous in member quantity while B more powerful individually, so as a group A and B are equally competitive. When two members within a group play a game to divide the payoff R, it is the same case as shown in the previous subsection. However, when the two players a and b are from different groups such that $a \in A$ and $b \in B$, they are backed by their own group, respectively. Because the power of the two groups is equivalent, the probability of winning in the situation of mutual fighting is $\frac{1}{2}$ again for both players.

The cost of mutual fighting c is assumed to depend on the economic condition of the society as in Ghatak and Verdier (2017). This makes sense in that c is the opportunity cost. When the economy is booming, the time spent on fighting rather than on work with higher wage is more costly. Vice Versa. c takes two values, $\bar{c}$ and $\underline{c}$ such that $0 < \underline{c} < \frac{R}{2} < \bar{c}$, to represent good and bad economic performance, respectively.

Another difference from the basic set-up of Ghatak and Verdier (2017) is that the cooperation reward r is contingent on whether the two players are from the same group.[31] One may associate r to sympathy or trust in similar people. If they are from the same group, the level of sympathy or mutual trust is higher, namely $r = \bar{r}$. They gain more happiness from cooperation and are more likely to develop beneficial network. Vice Versa. $\bar{r}$ and $\underline{r}$ are subject to $0 < \underline{r} < \frac{R}{2} < \bar{r}$.

In such a game, when the economy is in a good condition, namely $c = \bar{c}$, mutual fighting will never be a dominant pure strategy. If both players are from the same group thus bear higher sympathy, cooperation will be the dominant pure strategy; if they are from different groups thus have lower mutual trust, it will be the Game of Chicken. Nonetheless, when the economy turns down as in the Great Recession, i.e. $c = \underline{c}$, mutual fighting will always be a pure equilibrium no matter whether the two players are from the same group (a coordination game) or from different groups (The Prisoner's Dilemma).

This game model of heterogeneous socio-economic classes well fits the empirical results

[31] Ghatak and Verdier (2017) assume that the cooperating benefits vary depending on economic conditions rather than player heterogeneity.

in Section 4.5. In a prosperous economy, when people are unemployed, they do not choose to fight against elites who represent the high socio-economic class. Nevertheless, the Great Recession rendered job hunting difficult and wage declined, so fighting became less costly. Especially those lately losing their job during the Great Recession felt to be betrayed by elites and did not trust in them. Hence they were more likely to take the dominant strategy of fighting against elites by requesting the government to impose higher taxes on the wealthy and voting for left-wing populist who expressed anti-elite rhetoric.

4.D.3: Society of Heterogeneous Cultures and Identities

Let us take a recent immigration influx into account then: group A consists of natives who are the majority and group B is composed of recent immigrants who are the minority. The majority group is assumed to be more powerful so its member has a higher probability of winning, p_a such that $\frac{1}{2} < p_a < 1$, in a fight with a minority member. The sympathy or mutual trust is assumed to be at the lower level, namely $r = \underline{r}$, between a native and a recent immigrant because of differential cultural backgrounds and identities. The payoffs in this game of heterogeneous cultures and identities between a native and a recent immigrant is displayed in Table 4.D.2.

Table 4.D.2: Game of Heterogeneous Cultures and Identities

a \ b	F	NF
F	$(p_aR - c, (1-p_a)R - c)$	$(R, 0)$
NF	$(0, R)$	$(\frac{1}{2}R + \underline{r}, \frac{1}{2}R + \underline{r})$

When the economy is in a bad condition, i.e. $c = \underline{c}$, the majority member will always take the action of fighting since $\underline{c} < \frac{R}{2} < p_aR$. Moreover, if $\underline{c} > (1-p_a)R$, the recent minority member will surrender when the majority fights, which further improves the incentive of fighting for the majority. In such a game, even when the economy is in a good condition as the U.S. economy in 2014 recovered from the Great Recession, i.e. $c = \bar{c}$, the majority will still choose to fight so long as $\frac{R}{2} < \bar{c} < p_aR$. The recent minority will then correspond to concede since $(1-p_a)R < \frac{R}{2} < \bar{c}$. The notable difference from the previous subsection is that without recent immigration, fight will never be a dominant strategy in a booming economy. However, with recent immigration, fight will still be a dominant strategy for the majority in a prosperous economy if the majority is influential enough.

This game model of heterogeneous cultures and identities is consistent with the empirical results in Section 4.6. Amid and right after the 2014 immigration crisis, residents that were mostly affected became more negative to immigration even though the local labor market was not significantly impacted and the U.S. economy already largely recovered from the Great Recession. Furthermore, they were more prone to politically support right-wing populist who exploited anti-immigration rhetoric in his campaign.

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